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ORIGINAL ARTICLES.

ON THE DIFFERENTIATION, BY MEANS OF THE PITCH OF SOUND, OF PULMONARY SIGNS OBTAINED BY AUSCULTATION AND PERCUSSION.[1]

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OF NEW YORK.

This paper is intended to be a supplement to an essay "On Variations of Pitch in Percussion and Respiratory Sounds, and their application to Physical Diagnosis," to which was awarded a prize by the American Medical Association in 1852.[2] The introduction to that essay was as follows: "Very little attention has hitherto been paid to variations in the pitch of the sounds heard in the practice of percussion and pulmonary auscultation. The sibilant and sonorous râles, it is true, are distinguished from each other chiefly by a contrast in pitch, but as respects the remainder of the physical signs pertaining to pulmonary disease, they appear not to have been much studied in this aspect, and even the facts that have arrested notice do not seem to have been applied, save in a very limited degree, to physical diagnosis. By most writers on physical exploration, pitch modifications, except in the sibilant and sonorous râles, are not recognized, no allusion whatever being made to them.[3]

In the second edition of the able and comprehensive work by Dr. Walshe, of London, recently republished in this country, the subject is noticed more distinctly than by any other author with whose writings I am acquainted. Dr. Walshe enumerates among the elements involved in the different modifications of respiratory sounds in health and disease, variations in pitch; he also mentions several important facts with respect to these variations. But he apparently loses sight of their practical applications, making no reference to them in connection with the diagnosis of individual thoracic diseases. Barth and Roger state, as briefly as possible, that the bronchial respiration is higher in pitch than the cavernous. But, in general, as just remarked, nothing is to be found relating to this subject in standard treatises on auscultation and percussion.[1]

A single additional remark by way of introduction: The pitch modifications of sound, as before intimated, opening a field of study in physical exploration as yet but little cultivated, and to which, so far as relates especially to auscultation, my attention has been but recently directed, propriety and prudence dictate, not only caution in drawing deductions from a number of data somewhat limited, but a certain amount of distrust in a kind of observation in which the liability to error cannot be at once fully estimated. In view of these considerations, the conclusions which I shall present, are advanced as propositions to be confirmed by further researches, the object of this paper being, in a great measure, to invite the investigations of others in the same direction." The essay from which these extracts are taken, embraced the results of clinical studies in relation to the variations in pitch of the sounds obtained by percussion and auscultation. In these studies, the analytical method was pursued, that is, observations in healthy subjects and in cases of disease were recorded, and the records analysed. The diseases studied with reference to the pitch of sound, were pneumonia, pleurisy, pulmonary gangrene, pneumo-thorax, and phthisis in the different degrees and stages of this affection. Abridged histories of the cases of disease which were recorded and analysed, are contained in an appendix to the essay.

My interest in the clinical study of variations in the pitch of sound did not end with the publication thirty-three years ago, of the prize essay. The conclusions at that time presented with diffidence were confirmed by further study, and, together with additional conclusions, were submitted to the profession in my work on "Physical Exploration and the Diagnosis of Diseases affecting the Respiratory System," published in 1856, (1st Edition) and in subsequent publications. My object in this supplementary paper, is to give a statement of the differential characters derived from the pitch of sound, in the signs obtained by auscultation and percussion, as applied to the respiratory system, especially in so far as knowledge of these characters has originated in my own

[1]Read in the Section of Practical Medicine, Materia Medica and Physiology, at the Thirty-Sixth Annual Meeting of the American Medical Association.

[2]Vide the Transactions of the American Medical Association for 1852, Vol. V.

[3]The vocal sign ægophony should perhaps be excepted.

[1]In connection with this statement, it is proper to give the bibliography to which it applies. The works consulted are as follows: "Laennec," edited by Forbes; "Walshe on the Heart and Lungs;" Hughes's "Physical Diagnosis of the Lungs and Heart;" Barth & Roger's "Practical Treatise on Auscultation;" Gerhard on "Diseases of the Chest;" "Prize Dissertations on Physical Explorations of the Chest," by Dr. Holmes, Bell & Haxall; Blakiston on "Diseases of the Chest;" Latham on "Auscultation and Semeiology;" Swett on "Diseases of the Chest;" Bowditch's "Young Stethoscopist;" Lawson's lectures in the Western Lancet.

studies, and to inquire how far the results of my studies are, at the present time, accepted by clinical observers.

ON THE DIFFERENTIATION, BY MEANS OF THE PITCH OF SOUND, OF SIGNS OBTAINED BY PERCUSSION.

A conclusion deduced from observations in different diseases was that the sign known as diminish resonance or dulness is invariably higher in pitch than the normal vesicular resonance of the patient examined. This character of the sign is of practical use in determining a slight degree of dulness. Another conclusion deduced from the examinations of healthy persons with symmetrical chests, was that the normal resonance is higher, as a rule, at the summit on the right, than on the left side of the chest. The higher pitch on the right side, accompanies a normal relatively diminished resonance or dulness on that side. These conclusions have been generally accepted in this country, and to some extent by writers in other countries.

My observations subsequent to the publication of my prize essay led me to conclude that a tympanitic resonance or percussion is higher in pitch than the normal vesicular resonance. Statements by some writers are opposed to this conclusion; that is, it is stated that a tympanitic resonance may be lower in pitch than the vesicular resonance. Here, as in other instances in which there is a difference of opinion in relation to the relative pitch of sound, the question, of course, is to be settled purely by observation and discussion is therefore useless. I am quite sure, that in my own experience a tympanitic resonance has been always higher than the normal resonance of the person examined. The difference of opinion in regard to this point perhaps, arises from the lack of a sharp determination between pitch and quality of sound. In testing the judgment of a class of medical students as to the pitch of tympanitic resonance, I have found that a certain number pronounced the pitch of the sound to be lower when to my ear it is clearly enough raised. To disconnect pitch from quality requires in some persons practice.

The characters, aside from pitch, which distinguish the sign called tympanitic resonance, are by many not sharply defined. Precision requires as an essential characteristic of the sign, entire absence of the vesicular quality of sound. Intensity is not a character; the sound may be more or less intensified, and it may be weaker than the normal resonance. Quality and pitch are the sources of the distinctive characters of the sign. Every clinical observer who has given attention to the physical exploration of the chest, must know that there is an abnormal resonance which is more or less increased in intensity, as compared with the normal resonance, but which is not purely tympanitic. This kind of resonance accompanies pulmonary emphysema. It is the sign obtained over healthy lung above the level of liquid within the pleura, and over the sound lobe of a lung with a lobe solidified by pneumonia. An abnormal intensity is an essential characteristic of this sign. The other distinctive characters, are a combination of the tympanitic with the vesicular quality, and an elevation of pitch. Hence a descriptive name for this sign, is a vesiculo-tympanitic resonance. This name I proposed in 1856. The name has been generally adopted in this country and by some British writers.

We may formularize the facts relating to the pitch of sound in the pulmonary signs obtained by percussion, by saying, that (excluding flatness which is absence of resonance and therefore has no pitch) all the abnormal signs are higher in pitch than the normal vesicular resonance. This normal vesicular resonance varies considerably in pitch in different healthy persons. The variations will be found to correspond with those of the vesicular quality. In proportion as the vesicular quality of the normal resonance is marked, the pitch of sound is low, and *vice versa*. This correspondence is observed in the variations observed in healthy persons as well as in cases of disease.

ON THE DIFFERENTIATION, BY MEANS OF THE PITCH OF SOUND, OF RESPIRATORY SIGNS.

There are considerable variations in the intensity, the quality and the pitch of respiratory sounds in healthy persons. Hence, there cannot be an ideal standard of the normal respiratory or vesicular murmur. The intensity varies within pretty wide limits; the quality is in a greater or less degree, vesicular, and the pitch is not uniform. This statement applies alike to the sound of inspiration and expiration. Moreover, the length of the expiratory sound is variable.

The normal respiratory murmur varies also in different parts of the chest. The characters of the respiratory sound in the infraclavicular region differ considerably from those of the respiratory sound in the mammary, the axillary or the infra-scapular regions. Situated in either of these latter regions, the characters of the respiratory sound which are normal at the upper part of the chest would denote disease. And again the infraclavicular region does not furnish characters of respiratory sound which are alike on the two sides. A practical acquaintance with these normal variations is an important requirement as preliminary to the study of morbid respiratory signs. The importance of observing and recording the characters of the respiratory sound in a considerable number of healthy persons, and subjecting the observations to analytical study, seems not to have been fully appreciated heretofore, and is perhaps not sufficiently so by many of the present time. The conclusions obtained by this method of study, are embraced in my prize essay and in my "Treatise on Physical Exploration, etc.," the latter published in 1857. Without going into details, I will simply state that these conclusions have not only been confirmed by continued observations, but I am not aware that anyone has claimed to have disproved them.

Taking up first an important respiratory morbid sign, namely, the bronchial or tubular respiration, it is a correct comparison to say that the sound is like that produced by blowing through a tube. But how much more precise is a description embracing its characters pertaining to quality and pitch! These

characters are tubularity and raised pitch. The absence of any vesicular quality, and the high pitch are the essential characteristics. Intensity is of no importance. The pitch and quality, together with other characters relating to relative length, and the higher pitch of the expiratory sound, were stated in my prize essay as determined by observations in a series of cases of pneumonia in the second stage. With these characters, the sign always represents solidification of lung. I may add that the characters of the bronchial respiration, as determined by analytical study, prove this sign to be, not bronchial in its origin, but transmitted from the larynx and trachea. The proof consists in the fact, that the characters of the bronchial respiration are identical with those of the trachea and laryngeal respiratory sound.

Bronchial respiration represents complete or considerable solidification of lung. Now, in different diseases the lung is but slightly or moderately solidified. The bronchial respiration does not represent the latter physical conditions. These give rise to abnormal modifications of the respiratory sound not included in bronchial respiration. How are these modifications to be described and named? The names rude, rough or harsh respiration were heretofore used by English and American writers. These names express not merely an indefinite but a false analogy. As a result of clinical observations and analytical study, I proposed in 1856 the name broncho-vesicular respiration. The pertinency of this name consists in the fact that the characters consist of the normal vesicular and the bronchial in combination. These characters are combined in variable proportions corresponding to the degree of solidification. The name has been generally adopted in this country, although some writers prefer the name vesiculo-bronchial or versiculo-tubular respiration. Some English writers have adopted the name. But it has made as yet little progress in France or Germany. In France the name *rude* continues to be used, and in Germany the name introduced by Skoda, "indeterminate respiration," is still retained. It is difficult to understand why such an unsatisfactory adjective as "indeterminate" should be used to denote a definite physical sign, except that Skoda's authority in auscultation seems in Germany to be considered somewhat higher than that of Laennec.

It must be admitted that Laennec's description of a respiratory sign representing a pulmonary cavity (exclusive of amphoric respiration) was imperfect. Skoda denied the existence of such a sign. Of English writers, Walshe came nearest to a correct description. But there is a cavernous respiration, and that its characters, as derived from pitch and quality of sound, are sufficiently distinctive, was proved by facts contained in my prize essay. These facts consisted of recorded clinical and autopsical observations. The characters are a low pitch of the inspiratory sound, together with a quality which is neither vesicular nor tubular (a quality which I distinguish as simply blowing), and an expiratory sound still lower in pitch than the inspiratory. Elsewhere than in America, the present status of this sign is about the same as it was 35 years ago. Its non-existence is still the doctrine in Germany, and the description of it by English and French writers has not materially changed. As an illustration, at a meeting at Copenhagen last summer of members of a committee appointed by the International Congress in 1881 to establish uniformity of the nomenclature of physical signs obtained by auscultation and percussion, there were present a representative from England, from France, from Germany, from Denmark, and from America. The cavernous respiratory sign was one of the topics discussed, and so doubtful were the members of the committee present, other than the one from America, as regards the distinctive characters of this sign, that the member from England proposed to drop the sign from the list.

I do not hesitate to make the assertion that the characters of cavernous respiration are not less distinctive and reliable than those of any other auscultatory sign. Not only is its validity established on the sure basis of observations during life and after death, but the sign can be readily reproduced by inflating lungs containing cavities with flaccid wall, after their removal from the body. Moreover, it may be perfectly illustrated by a simple mechanical contrivance which represents the physical conditions giving rise to it.[1] Its distinctive characters will certainly be accepted anywhere as soon as the evidence of their correctness is brought to the notice and verification of clinical observers in other countries.

To the abnormal modifications of the expiratory sound which, remarkable as it seems, escaped the notice of Laennec and those who immediately followed him, attention was first directed by an American observer, James Jackson, the younger, in 1833. He pointed out the significance of a prolonged expiratory sound as a phthisical sign. He did not direct his attention to the pitch of a prolonged expiratory sound, nor have variations of this sound in this regard up to the present time received consideration elsewhere than in America. In this remark I refer especially to a prolonged expiration when it is either the sole respiratory sign, or not associated with pronounced abnormal characters pertaining to the inspiratory sound. The importance of observing the pitch of this sound was pointed out by me in 1856.[2] A prolonged, high pitched, tubular expiratory sound is as invariably proof of solidification of lung, when it exists alone or when it follows a normal inspiratory sound, as when associated with a high pitched tubular inspiration in the so-called bronchial respiration. If this prolonged expiratory sound be due to a prolongation of the act of expiration, as in cases of emphysema, or of some obstruction which occasions increased force of this act, the pitch is low and non-tubular as it is in health provided there be no solidification. The mere prolongation, therefore, of this sound, irrespective of pitch and quality, is not a sign of phthisis or any other affection which involves solidification. If not tubular and raised in pitch, a prolonged expiratory sound denotes either that the parenchyma of the lung is not solidified, or that the sound proceeds from a pulmonary cavity.

[1] Vide Lectures on the Physical Exploration of the Lungs, published in the Philadelphia Medical News, 1882.
[2] Vide Work on Physical Exploration.

In respect to the application of pitch to pulmonary râles (embracing under this term all purely adventitious sounds) one point only calls for remark in this paper. Of course, every auscultator knows the import of high and low pitched dry bronchial râles as regards their production in tubes of small or large calibre. The significance, however, of the pitch of moist bronchial or bubbling râles, inclusive of the so-called sub-crepitant râle, is perhaps not generally appreciated. A high pitch denotes that these râles are produced in tubes which are situated either within or proximate to solidified lung, and a low pitch excludes solidification at or near the site of their production. The significance of pitch in this connection was pointed out by Skoda. The correctness of his conclusions has been fully confirmed by my observations. This is a practical point in auscultation to which most writers make no reference.

ON THE DIFFERENTIATION, BY MEANS OF THE PITCH OF SOUND, OF VOCAL SIGNS.

To those who have given no attention to the subject, it may seem that the differential characters of vocal signs offer little room for variations in the pitch of sound. The fact is otherwise; the variations in pitch are of much importance. An abnormal increase of the intensity of vocal resonance without any notable elevation of pitch, as compared with the normal resonance, denotes either a moderate degree of solidification of lung or a pulmonary cavity. The resonance of the voice transmitted through a cavity may be greatly intensified, so much so as to be painful to the ear, especially if the bin-aural stethoscope be used, whereas the pitch is not much, if at all, râised. Taken in connection with other signs, a notable increase of resonance, without elevation of pitch, within a circumscribed area is highly significant of a cavity. If the pitch be high, either with or without increase of intensity, the resonance denotes complete or considerable solidification of lung. This elevation of pitch is in fact an essential characteristic of bronchophony. The voice of the patient seems to be near the ear. Doubtless this was meant by Laennec when he described the voice as being conducted up the stethoscope. A high pitched vocal resonance near the ear is always evidence of complete or considerable solidification, whether the vocal sound be intense or weak. Increased vocal resonance and bronchophony are, therefore, two distinct signs differentiated especially by pitch, the significance of the former being either moderate solidification or a cavity, and that of the latter being complete or considerable solidification.

The sign called by Laennec ægophony, and to which he devoted so much consideration in his treatise, has the pitch of bronchophony. It denotes considerable or complete solidification. In this respect it is neither more nor less than bronchophony. It differs from the latter in an apparent distance, as well as in its tremulous or bleating character. In pectoriloquy, as is now well known, in opposition to the teaching of Laennec, the speech may be conveyed either by solidified lung or through a cavity. Can it be determined whether or not pectoriloquy denotes a cavity or solidified lung in particular cases? I answer this question in the affirmative. The discrimination is made by attention to the pitch of sound. If the vocal resonance associated with the transmission of speech has the characteristics of bronchophony, that is, the pitch raised and the voice near the ear, the medium of transmission is solidified lung; if, on the other hand, the characteristics of bronchophony be wanting, the transmission is through a cavity. This method of differentiation I have practised and taught for many years. Its reliability is yet to be accepted by others.

It is noteworthy that the auscultatory signs, respiratory and vocal, together with those obtained by percussion, are either single or compound. Percussion furnishes a combination of vesicular and tympanitic resonance in the sign called vesiculo-tympanitic resonance. The respiratory signs called vesicular and bronchial are united in the broncho-vesicular respiration. Pectoriloquy may be associated with either bronchophony or cavernous resonance. Other combinations not considered in this paper are broncho-cavernous and vesiculo-cavernous respiration.

DIFFERENTIATION, BY MEANS OF THE PITCH OF SOUND, OF SIGNS PRODUCED BY THE WHISPERED VOICE.

In 1856 I described certain signs produced by the whispered voice, and proposed names for their designation. Subsequent observations led me to form a group of these signs corresponding to those produced by the loud voice, and corresponding names were applied to them. An enumeration of the morbid signs, designating the normal sound the bronchial whisper, is as follows:

1. Increased bronchial whisper.
2. Bronchophonic whisper or whispering bronchophony.
3. Cavernous whisper.
4. Amphoric whisper.
5. Whispering pectoriloquy.

Variations in the pitch of sound constitute an essential element in the differential characters of these signs.

1. A simple increase of the normal bronchial whisper is but little raised in pitch, the chief characteristic being an abnormal intensity. This sign corresponds to increased vocal resonance.

2. Bronchophonic whisper corresponds to bronchophony with the loud voice, and a high pitch is its chief characteristic.

3. A cavernous whisp r is low in pitch. Its analogue, as a vocal sign, is an intensified resonance without elevation of pitch.

4. An amphoric whisper is characterized by a musical intonation.

5. Whispering pectoriloquy may denote either a cavity or solidified lung. If the latter, the pitch of sound accompanying the transmitted speech is high; if the former, the pitch is low.

As a whisper is an expiratory sound, the characters of these several signs correspond of course to those produced by the act of expiration in the respiratory signs which have the same significance, namely, the broncho-vesicular, the bronchial, the cavern-

ous and the amphoric respiration. Whispering pectoriloquy, alone in the group of whispering signs, has no analogue in the respiratory signs. An experience of many years has led me to consider the group of signs produced by the whispered voice as an useful extension of auscultation.

AN ATTEMPT AT THE RADICAL TREATMENT OF TUBERCULOSIS.[1]

BY J. T. WHITTAKER, M. D.,

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Pathologists recognize the frequency of recovery from attacks of tuberculosis of the lungs. When patients once thus affected die of other or intercurrent disease, characteristic cicatrices or calcareous condensations are found in the lungs, along with adhesion of the opposed pleura.

It would therefore seem rational to endeavor to induce or hasten such processes during the course of the disease, with the hope of destroying its cause (perhaps by preventing the nutrition of it), or at least to oppose a bar to its further progress. The possibility of such a procedure would seem all the more justifiable for the reason that the disease is and remains, in most cases, for a long time quite strictly circumscribed. It may be taken for granted that every one now believes the disease to be a mycosis. With the double purpose of directly addressing the cause of this disease with an antimycotic agent, and of producing the irritation which might result in proliferation of the connective tissue of the lungs, I have made a number of parenchymatous injections into the lungs with solutions of the mercuric bichloride of varying strength. A number of experiments of similar character were made by Hiller, of Berlin, with negative results, but I have been stimulated to repeat these experiments with the modification in the solution suggested by Stern, in the hope that by rendering the remedy less irritating to the lungs, I might be able to use a larger dose.

I took the precaution, in making these experiments, to select only such patients (with one exception), in whom the disease existed in what is commonly called its first stage, with at most apicial consolidation of the upper lobe. I did not venture upon the use of this remedy where undoubted evidence of softening existed, or where cavities could be appreciated in the lungs. Seven years ago I had the misfortune to lose, upon the operating table before the class, a case in which I had aspirated the contents of a large cavity in the left upper lobe.[2]

The experiments of Wilhelm Koch upon cats and dogs, which consisted in making into the lungs parenchymatous injections of potassium iodide, with the effect of producing proliferations of the connective tissue to any desired extent, relieved me of the necessity of making any preliminary operations of this kind myself, and the impunity with which large doses of the bichloride are introduced subcutaneously in man, relieved me of any anxiety of danger in this respect.

[The essayist here related his observations upon five patients carefully selected, where accurate thermometric records were kept and all points of history noted with regard to cough, night-sweats, appetite, condition of bowels and weight. The bichloride was injected daily at a depth of four to six inches, in quantities varying from 1-32 to ⅛ grain. At the same time all the patients inhaled from an atomiser a solution of the bichloride with common salt three times daily for five weeks. There was, as a result of this treatment, no change whatever in the course of the disease. What fluctuations were observed were of the same character precisely as before. The essayist next spoke of the value of the bichloride as an antimycotic agent, and sought to be able to account for its failure in tuberculosis.]

It is the testimony of all competent mycologists, without exception, that the bichloride of mercury is the most potent antimycotic which we possess. In comparison with this agent, observes Küster in his paper on the treatment of wounds (Real-Encyclopädie, xv, 290), "all others must stand in the background, and it is incomprehensible," he continues, "why the search for new antiseptics continues with the risk they imply of unknown dangers to the patient." Schede, of Hamburg, concluded his discourse at the surgical section of the International Congress at Copenhagen, in August last year (vide, published in *Volkmann's Sammlung*, Feb. 12, 1885), with the statement: "Whoever will use sublimate after the manner mentioned, with regard to the cautions described, will find it by far the most reliable and effective means of disinfection, and not only this, but also the agent which will favor the quickest healing of wounds. Moreover, it is the most free of all agents of any disturbing or dangerous accessory effects." Bruns (*Berlin klin. Wochenschr.*, xx, p. 295, 1883) says that with sublimate antisepsis "cases of so-called aseptic surgical fever almost never occurred." The obstetricians and gynæcologists at once adopted the bichloride with such degree of enthusiasm as to have led to its abuse and to reactionary protest in the latest times. Fuhrman, of Breslau, shows by statistics (*Schmidt's Jahrbücher*, June 24, 1884), the superiority of sublimate over carbolic acid in the effect on the temperature curves of puerperal fever. In Kezmarsky's clinic at Buda Pesth a comparison between the results of treatment with carbolic acid and sublimate showed "decidedly better results for sublimate," as parametritic exudations, ulcerations, etc., etc., occurred much more rarely. It is useless to multiply statements here as to the supreme value of the bichloride in antisepsis. Thorn, in his paper, "A View against the Present Method of using Sublimate" (*Volkmann's Sammlung*, Feb'y 12, 1885), quotes from no less than twelve authorities, who have expressed themselves for the most part emphatically in its favor, though this author himself is decidedly opposed to its use in obstetrics.

[1] Read in the Section of Practical Medicine, Materia Medica and Physiology, of American Medical Association, May, 1885.

[2] I may state, parenthetically, that I had upon this occasion to appear before a coroner and fortify my position with the citation of an array of cases in which this operation was successfully performed, though, as was later ascertained, with no permanent benefit in any case.

The surgeons and gynæcologists who have made such extensive use of the bichloride, have employed it, as a rule, in the strength of 1 to 1,000; in round numbers, seven grains to the pint. In this proportion the corrosive sublimate is absolutely fatal to all germs in the free state. Whatever may be the doubts or dangers of its internal use in this strength, it is conceded on all sides that in this proportion sublimate remains the best agent for the disinfection of all the externals of surgery, as of the hands, instruments, etc. But corrosive sublimate has been given internally for all time in much stronger proportion. Perhaps the one-eighth of a grain is considered the average full dose of the remedy internally. According to Seidel (Handbuch der Gerichtl. Med., Bd. 11, 1882), the maximal dose of the bichloride of mercury is the 9-20, a little less than one-half a grain, and the maximal quantity that can be safely administered in the 24 hours is 1½ grains. Severe toxic effects have been observed after the exhibition of 1.8 grains, a quantity which most authors consider a fatal dose. The real lethal dose, this author continues, may be put somewhat higher, viz., at 3¾–7½ grains, though recovery has been observed after 15 grains taken on a full stomach, and oftener is incidentally remarked than after toxic doses of arsenic. Eulenberg (Handbuch der Allg. Therapie. Bd. 1, 2, 3, p. 105, 1880) gives a solution of 1 to 100 as the proper quantity for percutaneous use in the treatment of syphilis, of which solution 15 drops, containing 3-20 of a grain, is the proper dose in the treatment of syphilis by this method. Single doses of 3-10 to 9-10 of a grain may, he says, according to Seine, produce severe toxic effects. As a rule, the average dose cited, 3-20 grain, produces no local irritant effects of any severity, but should they ensue, they may be obviated by combining the sublimate with morphine. When local caustic effects are desired, very much stronger proportions have been used. Thus Bienfait, of Rheims, injected into the œdematous eyelids of milzbrand carbuncle an alcoholic solution of 1 to 5; Luton, a few drops of an alcoholic solution of 1 to 30; Dominguez, in elephantasis Grecorum, 20-80 drops of a 10 per cent solution, equal to 8-10 of a grain. But to return to the disinfecting or antimycotic solutions. To render the bichloride less painful and most permanent, as well as to secure a preparation most easily absorbed and assimilated, it has been proposed by Stern, Recler and Gschirhakl, to make of it a neutral solution by the addition of common salt, in the proportion of ten parts of salt to one of the sublimate. And, in fact, according to Bernatzik (Real-Encyclopädie, vii, 33), the addition of salt renders the preparation less irritant, and according to Sigmund, less productive of inflammatory depots. It is highly probable that mercury, administered in whatever form, is converted into the bichloride and enters the blood as an albuminate, with the liberation of the chlorine in the blood. The experiments of Mixerliez have shown that the albuminate of mercury has by no means the same anti-nycotic property as the sublimate, and is not even as effective as carbolic acid; and the observations of Schill and Fischer, published in the communications of the sanitary office at Berlin, showed that corrosive sublimate did not destroy the virus or germs of tuberculous sputa as effectively as carbolic acid. The most satisfactory experiments in this direction were made by Hollaender (*Deutsche med. Wochenschrift*, April 21, 1884). The sputum was taken directly from pronounced cases (the presence of bacilli having been always previously established), and after remaining half an hour in contact with the disinfecting substance, was rubbed up fine in a porcelain mortar and injected into the trachæ or peritoneum of rabbits. As the result of 23 experiments, it was observed that small quantities of no disinfectant sufficed to render tuberculous sputum innocuous. Sputum which contained one-tenth per cent. sublimate produced upon the peritoneum a granular eruption, which could be regarded as tuberculous, though no bacilli could be demonstrated. To absolutely destroy all tuberculous germs, it was necessary to use a proportion of two-tenths of one per cent. corrosive sublimate. To produce the same effect with carbolic acid required a strength of 3 per cent. Two-fifths of one per cent. equals practically one grain to the ounce. A hypodermic syringe full of this solution contains about one-eighth of a grain. This quantity can be injected with impunity into the lungs. But the question arises whether the destruction of a large mass of the bacilli tuberculosis will cure the disease. Will not a few which must, under any circumstances escape, continue to multiply as before? Or will not the soil remain just as fertile to the reception of new seed? The mycologists themselves do not, as a rule, look with favor upon experiments of this kind. Yet clinicians can never be content with prophylaxis alone. There is still hope of effecting, by drugs, such chemical change in the lung tissue as will make it infertile to the growth of the bacilli, as will bring about the condition which takes place in the process of natural recovery.

TREATMENT OF THE SECUNDINES IN ABORTION AND IN LABOR.[1]

BY W. H. WATHEN, M.D.,

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The management of the membranes in abortion and in labor has recently received a good deal of attention by the medical profession of this country and of Europe, with a very marked improvement in the treatment. Still we are very far from being united upon any one course of practice; some physicians adhering to the purely expectant treatment; some practicing the immediate removal of the membranes, while others adopt a system intermediate between these. We cannot, in the time allowed, consider this subject in detail, but hope to make some practical suggestions, based upon experience and observation in the treatment of these cases.

[1]Read before the Section of Obstetrics and Diseases of Women, of the American Medical Association, April 28, 1885.

We will first speak of the membranes in abortion at the different periods of pregnancy, from the beginning to the end of the seventh month, and then of the membranes in premature labor, and in labor at term. That we may treat abortion intelligently and scientifically, we should fully understand the formation and the relations of the fœtal and the maternal membranes, as the treatment indicated at one period of pregnancy may be contra-indicated or unnecessary at another.

In an excellent paper by Dr. David Ingles, of Detroit, in the April number of the *American Journal of Obstetrics*, the following appears: "During the first two months of gestation the fœtus and the placenta are so intimately bound together that an abortion at that period usually means that when one is expelled the other must be also."

Every physician should know that in the first and second months of pregnancy there is no placenta, and that in abortions before the eighth week the fœtus is surrounded only by the amnion, the chorion and the decidual membranes, and that the formation of the placenta proper begins at the decidua serotina about the end of the second or the beginning of the third month; until this time the fœtus is nourished through the villi of the chorion, which enter alike into all parts of the decidua reflexa. In abortions prior to this date there is no placenta to remove, and the membranes, if retained, are generally innocuous, and will soon be separated and expelled without causing hæmorrhage, septicæmia, or any sort of polypoid formation; nor will involution or convalescence be interfered with. If these membranes can be reached by the finger in the vagina and removed with comparative ease, they should be taken away to avoid possible trouble, but when they cannot be removed without introducing the finger or instruments into the uterine cavity, they had better be retained. I have seen no ill effects from this practice. In cases where serious results and death have been reported from the retention of these little membranes, I suspect the pregnancy had continued long enough for the formation of a placenta.

In abortions from the beginning of the third to the end of the seventh month with retained placenta, with the os dilated or firmly contracted, the membranes should be immediately removed when we are convinced that nature will not do so within thirty to sixty minutes; for if not expelled in this time, we are unable to form an intelligent idea of how long they may be retained, or when some complication may arise. The advocates of waiting for nature in these cases are illogical, and the practice has cost the lives of many women. We can argue with more consistency against any interference in the first or second stages of abortion or labor, for as the dangers of complications from delay are greater in the second than in the first stage, so also are the dangers from delay in the third stage greater than those of the second. When nature fails to do her part promptly and perfectly, we should assist just as in any other abnormal condition of the system.

We might say, with equal propriety, that chloroform or the forceps should never be used in labor. A retained placenta may be expelled at any time without serious results, but it is sometimes retained for months, and until expelled, the woman's life is constantly in jeopardy from hæmorrhage, inflammation, septicæmia, etc. Medical literature contains numerous instances of serious complications, and of death from a retained placenta in abortion; and doubtless most physicians who enjoy a large practice have had similar experiences. I have seen many cases of uterine disease caused by retained membranes; one woman died, and several others narrowly escaped death. The immediate removal of the secundines tends to prevent these complications without substituting others in their stead. With the proper facilities, and with reasonable degree of care, the removal of the membranes with the finger, or even with instruments, will seldom be followed by any bad results, nor do I remember instances in my experience, or in medical literature, that contravene this opinion. I can see no excuse for delay; the complications of the expectant treatment are nearly always more serious than those following the immediate removal of the membranes.

The operation is not usually difficult, and if done without delay, the os will generally be enough dilated to admit the finger or the curette. But if the os is contracted, it can be easily dilated to admit the finger, etc. If this cannot be done by the finger, we can use a large metallic dilator—such as my modification of Leonard's[1]—which is preferable to any kind of tent. With the finger we can separate the membranes and remove them from the uterine cavity without inflicting injury more easily than by other means. The finger can often be introduced to the fundus by pressing down the uterus from above, or by drawing it down with a vulsellum; but the hand may be introduced into the vagina, when the finger or fingers will easily reach to all parts of the uterine cavity. There may be cases in primiparæ in the early part of pregnancy when the introduction of the hand into the vagina might lacerate its walls. Then we may remove the placenta with the curette and the placental forceps. There should be no excuse that we have no curette—in default of anything better, one can be readily improvised by using a copper or iron wire bent upon itself in a bow, after the fashion of this I show you. It can be made of any size, and will serve our purpose nearly or quite as well as those handsomely manufactured.

The fœtus may be expelled with the amnion intact, but the chorion, the decidua, and the placenta are retained. We may fail to understand this condition or to treat it successfully, for most of our text-books do not sufficiently explain it. Most authors teach that when the fœtus is expelled in an unbroken amnion the entire product of conception is usually expelled with it. This is not correct. I have had several cases of abortion, at three or four months, where the sac was expelled intact with the placenta retained; and Dr. Sawyer reports a similar case in an abortion at five months. This occurs before the amnion and

[1]This dilator is manufactured by Messrs. Tafel Bros., of Louisville, and is superior to any other instrument that I have seen for rapid dilatation of the cervix in abortion or at other times. It can be had at about one-third the cost of Ellinger's.

the chorion are firmly united, the cord being severed between the amnion and the chorion without in any way interfering with the integrity of the amnion around the fœtus. We should examine carefully into these cases to see if pregnancy has not continued more than two months, as we cannot be always guided by the opinion of the patient. Women are often unable to state with much accuracy the date of impregnation. Nor can we tell positively the date of conception from the size or length of the embryo. I have seen an embryo not more than an inch long with a a placenta one and a half inches in diameter.

Authors are not yet agreed as to the best treatment of the third stage of premature labor, or of labor at term. Some recommend the purely expectant treatment, and will use no means to remove the placenta, though it be retained for hours or for days. I have no respect for the opinion of those who advocate this treatment. They destroy their own arguments, for while they have no concern about a retained decomposed placenta, they see great imaginary dangers from the retention of a few membranes occasionally left in the uterus after expression. Argument is unnecessary.

When the placenta is not expelled within thirty minutes after the birth of the child, we should then assist nature in getting rid of it. It may not be necessary to do more than to induce contraction by gentle kneading or massage over the uterus. But it is safe treatment to place a hand over the uterus immediately after the expulsion of the child to see that contraction occurs. If there is a tendency to relaxation and hæmorrhage, the uterus should be firmly kneaded until it contracts. I do not believe there is anything gained, when there is no complication requiring it, by attempts to remove the placenta by a *vis a tergo* or a *vis a fronte*—by expression or drawing upon the cord, singly or combined—until twenty or thirty minutes after the expulsion of the fœtus. It is true that Credé reports that he removes the placenta in four and a half minutes after the birth of the child with almost universally good results; but this treatment is unnecessary, may prove prejudicial to the interests of the woman, and will never be generally adopted. Garágues reports over four hundred cases in which the placenta was removed within ten to twenty minutes with excellent results. Those who oppose expression, and those who picture great dangers from pulling upon the placenta, draw conclusions not sustained by facts. But drawing upon the cord is unscientific, and puts the placenta in an unnatural shape that tends to obstruct its delivery. It folds it upon its maternal surface, like an inverted umbrella, and presents its longest diameter in the os, while nature expels it folded upon its fœtal surface, with its shortest diameter in the os. It is more scientific to introduce the fingers or the hand into the vagina and draw upon the presenting edge of the folded placenta. If it is not adherent, it can in this way be easily removed and inflict no injury upon the uterus; but if adherent, the hand should be introduced into the uterus and separate and remove the placenta. There is no impropriety in supplementing this force with expression, or in supplementing expression with this. The proper treatment is to express during contraction with one hand and to pull gently on the placenta with the other. There is no danger of inverting the uterus, or of injuring it, by either or both of these means, if carefully and scientifically practiced. If the uterus becomes indented by expression or drawing the hand upon the abdomen, or the hand in the vagina would promptly detect it and prevent inversion.

Hæmorrhage in the third stage of labor is not controlled entirely by the general muscular contraction of the uterus and by the contraction of the muscular fasciculi surrounding the uterine vessels, but also by the formation of coagula in the utero-placental vessels. If we remove the placenta by expression or by pulling within less than fifteen to thirty minutes after the birth of the child, it is probable that coagulation may not be complete. Where the placenta is firmly adherent, and there is a tendency to tonic spasm of the entire uterus, or in cases with irregular and firm contractions of the circular fibres about the neck, imprisoning the placenta, we should immediately introduce the hand into the uterus and separate and remove the membranes. Delay is dangerous, and often increases the difficulty.

I have recently had two cases of tonic spasm of the whole uterus; the first with adherent placenta, which I promptly separated and removed. I was called at noon to see the other patient, who was delivered at ten o'clock the preceding night. The attending physician told me that the uterus contracted firmly as soon as the child was born, and that he could not get the placenta away. The bladder was enormously distended with urine, and the uterus was in a state of tetanic spasm, aggravated by the persistent use of dram doses of fluid extract of ergot. It could scarcely be indented through the abdominal walls, and the point of the finger could not be introduced into the os. The pulse was 100 and the temperature 101°. Fearing that an attempt to remove the placenta would not only be unsuccessful, but would also endanger the uterus, we delayed our efforts until four o'clock P.M., but stopped the ergot and gave morphine hypodermically. When we returned the uterus was less firmly contracted and the os more patulous, but very hard to dilate. Her pulse was 130, temperature 104°. She was thoroughly chloroformed, and the os enough dilated to enable me to get hold of the edge of the placenta and remove it. She was free of fever the next morning, and made a speedy recovery.

The hands and instruments used in removing the placenta should be thoroughly cleansed and disinfected before they are introduced into the vagina or the uterus. All instruments should be immersed in boiling water. If we practice all necessary precautions, and remove the membranes with care, the dangers from direct violence and from septicæmia would be reduced to a minimum.

DR. JAMES R. CHADWICK, of Boston, said he indorsed the views expressed by Dr. Wathen, but that no rule could be formulated applicable to all cases. In abortions before the formation of the placenta,

he regarded the history of the case, and if the woman was a "bleeder" he immediately removed the decidual membranes. He never allowed a placenta to be retained. He had never seen a placental forceps that was of the slightest value. The finger was the best dilator, and the best instrument to remove the membranes.

Dr. Green, of Kentucky, pursued the immediate course, and gave the history of a case in which he removed the retained membranes in an abortion at four months.

Dr. Sinclair, of Boston, thought the secundines ought to be removed at the earliest possible moment, and agreed with the author of the paper and with Dr. Chadwick, that manual dilation of the cervix is the best method. When the os is contracted, the fibres will usually yield to the finger in from six to eight minutes.

Dr. Carroll, of Texas, was in favor of the immediate removal of the membranes, and thought that the retention of decidual shreds, or a small clot of blood, may cause alarming hæmorrhage. He would not hesitate to use a metallic dilator, but thought the finger a better instrument.

Dr. Wathen closed the discussion by saying that he advised the use of his modification of Leonard's dilator only in extreme cases in which the finger had been employed without effect.

INTRAPERITONEAL ADHESIONS IN RELATION TO TAIT'S OPERATION.[1]

BY B. E. HADRA, M.D.,

OF SAN ANTONIO, TEXAS.

The following lines are in no way intended to make war on Tait's operation; such a course would be folly on my part. But where there is so much uncertainty as regards diagnosis, treatment, and the *modus medendi* of these operations, any light thrown on the subject from whatever source should be considered worthy of attention. No doubt, when the ovaries or tubes, or both, are so diseased as to preclude all hope of recovery, their removal is indicated. But reading every day the remarkable successes of these operations, even where the uterine appendages are not diseased or at least not materially changed, and that exploratory laparatomies result in surprising improvement, we must therefore search for something more tangible, and more in accord with pathological principles, than the questionable mental impression upon the hysterical woman.

The objective point in this paper is the importance of adhesions inside of the peritoneal cavity. It would be but an old story to call attention to those gross adhesions, those which may glue together any two or more of the intra and extra peritoneal tissues and organs. "The most frequent change is due to a chronic adhesive inflammation (perimetritis, perioophonitis, etc.) Loose or firm pseudo-membranes are stretched out between the posterior aspect of the uterus and the anterior surface of the rectum, or the sides of the true pelvis. Others glue the tubes and ovaries to the rectum and pelvis or uterus, and in this way the tubes often become bent, or closed, the ovaries become displaced, and often thus become imbedded in pseudo-membranes, and only with the greatest difficulty can they be found," etc. (Orth, Pathol. Anat. Diagnostic, 1876.) Of course, any operator understands this, and has seen such cases. In calling attention to those gross adhesions, before coming to the leading point; it is not to offer anything new, but to give a word of caution against such adhesions which, if overlooked, might induce the operator to remove the uterine appendages in cases where there is no need for their removal. I will cite here a case reported by Mr. Tait himself, in the *Med. Times and Gaz.*, July, 1884: "Pains in pelvis since birth of last child. Fluctuating tumor felt in Douglas' sac. Tubal tumor suspected, which tumor turned out to be an accumulation of pus. Laparatomy: an intestinal loop firmly fixed to pelvis. It was freed with complete recovery." I myself observed the following case: F. K. Puella publica, æt. 48, suffered for many years, addicted to morphine, weight about 250 pounds; evening exacerbations 101. Purulent discharges from womb; painful spot as large as two hands in left epigastrium, on percussion over which there was indistinct dullness. Defecation extremely painful, and the bowels could only be moved by strong purgatives. This indeed was the main trouble. Palpation over the parametrical spaces gives a dull and undefined elastic touch. Diagnosis: Pyosalpinx. Operation: Only small area of uterus visible; all around it firm adhesions to the abdominal walls. The left tube, which was then opened by a rectangular incision, was found filled with pus; in the bottom there was an opening large enough to admit the finger, which communicated with the bowels. This fistula was closed and a drainage-tube inserted into the tubal sac. The patient here manifesting symptoms of exhaustion, the operation was terminated. She died in about 12 hours. Post-mortem examination showed the same condition in the other tube. But in addition, all the abdominal and pelvic viscera, the womb, tubes, ovaries, bowels, etc., were found agglutinated to such an extent that it had to be exsected entire, when it was found that the intestine was adherent to the tubes in no less than five different places. Although there was a double pyosalpinx, the adhesions of the intestine were the cause of the main sufferings.

Another case which I saw in Dr. Cupples' practice, of San Antonio, is unique in many respects. A German woman, æt. 23, married three years, began to suffer four months after marriage with severe pains in abdomen; she had dysmenorrhœa, the result of a gonorrhœa contracted from her husband. Her menses became more and more scant, until finally the function wholly ceased, when she became a confirmed invalid, being for the most part confined to her bed. She was much emaciated, suffered greatly with painful defecations and obstinate constipation, the bowels moving only with active purgatives. A manual examination revealed very painful abdomen, espe-

[1] Read in the Section on Obstetrics and Diseases of Women, at the Thirty-Sixth Annual Meeting of the American Medical Association.

cially in left hypogastrium; womb retroflexed, firmly attached and immovable; cervix atrophied and sacro-uterine ligaments so swollen and thickened as to resemble a rectal stricture.

Laparotomy was performed. A coil of intestine crossed the fundus uteri, and adhered to it. It was detached with some force. Other coils were attached to both sides of the uterus, but were so firmly adherent that after considerable manipulations it was decided to terminate the operation, as farther efforts to sever the adhesions would greatly have endangered the patient's life. Much to our surprise, she made an uninterrupted recovery. All pains disappeared, her bowels became regular, menstruation became normal, and she is now doing her own household work, and fulfilling all the duties of a married woman.

I have selected these cases which might be greatly increased from the daily medical literature, to illustrate the fact that adhesions, especially those involving the intestine, are in many cases the real cause of the trouble, and that they are liable to induce the operator to perform Tait's operation. A limited peritonitis, caused by the introduction of obnoxious or virulent elements through the tubes, may at any point lead to adhesions which might be very slight, yet interfere sufficiently with the functions of the different organs to justify the thought of the operation, and not only bowels, but also ovaries and tubes may easily be disturbed in their action by frail and seemingly insignificant threads. The conclusion to be drawn is obvious. In making Tait's operation, the closest attention should be paid to searching for the above named changes, as it might even happen, that those bands be broken up without attracting the notice of the operator. When the appendages are found healthy or but slightly changed, we should make a thorough examination of every organ, each for itself, which might enable us to find a solution of the mystery in detecting adhesions somewhere where we might have expected them the least.

But the object of this paper goes farther; it is to call attention also to the adhesions inside of the peritoneal cavity, *above the pelvic organs*, and especially between omentum and parietal and visceral peritoneum. A peritonitis once set up, is liable to deposit its poison anywhere within the sac, and to cause circumscribed adhesive inflammation anywhere. This fact, which is well known to the pathologist, has been somewhat neglected by the gynecologist. A woman has an inflammation after dysmenorrhœa or in childbed, not severe enough to claim the dignity of peritonitis. Nevertheless it is such, and after a while, when she has been pronounced well, she will complain of pain in the abdomen, either all over or only in a limited spot, most generally in the left epigastrium. The bowels become flatulent, which may form the main complaint. These symptoms grow in intensity until we are induced to make Tait's operation, when we are surprised to find the uterine appendages normal. Sometimes massage, which we know can cure adhesive inflammation, will give relief.

Looking over our authorities, we find many ingenious explanations for the pain in the epigastrium; by nervous anastomosis, etc., but in reality it is nothing more than a chronic adhesive inflammation. The flatulence of the bowels is also easily explained by the participation of a smaller or larger portion of their peritoneal covering. By percussion in some cases we can map out the area of circumscribed adhesive peritonitis. It will give a slight dullness, even assume the appearance of a tumor. We can further understand that the peritoneal coverings of liver, spleen and stomach become involved, and in looking through Tait's list of laparotomies, we can readily see that the marvelous cures of liver and spleen affections are the result of unavoidably breaking up adhesions in the attempt to examine those organs. A case, the history of which I will give below, taught me that in some forms of latent peritonitis are strings of lymphatic nature which run between the different surfaces of the peritoneum, and which filaments, I am satisfied, shrink up after death, but during life are the vehicles of lymph or other plastic and irritative material. These lymphatic strings might be the first stage of the adhesive peritonitis, glueing together different structures and surfaces, or they might persist in this form and give rise to many of the most common complications enumerated before.

The history of this case upon which I based my deductions is as follows: A Bohemian woman, married eight years, has had two children, the last three years ago, since which time she has been an invalid. Menstrual function has not been resumed since. Constant pain over the entire abdomen, especially in left side. On examination, womb was found simply atrophied—nothing else. Laparotomy; about two tablespoonfuls of dark serum; uterus very small, ovaries normal, tubes seemingly healthy (afterwards found closed and mucous membrane thickened). Ovaries and tubes were removed, as Tait's operation was intended. Still, however, having my mind on the mentioned peritoneal adhesions, I stated to my friends Drs. Cupples, Tyner, Watts and Kingsley, who very kindly assisted me, that I would search farther up in the peritoneal sac. I introduced my hand under the omentum and swept it over the whole anterior surface of the bowels, repeating this maneuvre three or four times, and each time my hand was *covered with a large number of transparent filaments, resembling cobwebs*. The abdomen was then closed, and the woman made an uninterrupted recovery.

The case which first attracted my attention in this matter, and from which my opinion was formed, occurred in Dr. Cupples' practice, a few weeks previous to the one just reported.

A lady, 55 years of age, presented the usual abdominal symptoms. The so-called ovarian regions were very painful on pressure, and there was a spot above the umbilical horizontal, where there were constant throbbing and cutting pains, preventing her from lying on that side. Had passed menopause. Tait's operation. Tubes and ovaries normal, except a few pea-like cysts in the latter, as formed so often. The hand of the operator was then made to sweep over the bowels for examination's sake. Patient made a good recovery, and experienced immediate relief from the painful spot in her side. In this instance, I could not believe the removal of the appendages had anything

to do with the speedy cure, and concluded that by the manipulation an adhesive process had been broken up.

It might seem unwarranted to draw from so little personal experience such decided and important deductions. However, as it so often happens in Tait's operation, that we find the tubes and ovaries sound, or not sufficiently diseased to account for the mischief, and that their removal cannot explain the speedy relief which follows, I hope in view of these facts, that my observations will be accepted as worthy of consideration. Should these views become verified by further experience, we will then proceed in about the following way: Laparotomy; minute examination of all the pelvic viscera with special attention to adhesions; breaking them up; insinuating hand upwards with sweeping movements between omentum and bowels, and between omentum and parietal peritoneum. These movements have to be made thoroughly, sweeping over all the surfaces, especially the side and spot where was most complaint. Thus a new operation—freeing the peritoneum throughout its entire area—will have been performed, and I hope, with the fullest benefit. Particularly should this operation be tried in young women, to save, if possible, the functions of generation, which consideration should warrant the risk of a second operation, being so little danger in the first one.

As stated in the outset, my object is not to disparage Tait's operation, but in doubtful cases, when we find the ovaries and tubes healthy, let the unfortunate woman have the benefit of the doubt, and let the operation conform to the conditions which are revealed by it.

MEDICAL PROGRESS.

ANATOMY AND PHYSIOLOGY.

THE CHOLERA-BACILLUS.—(In the JOURNAL of June 13th we gave a *résumé* of MR. WATSON CHEYNE'S methods of cultivating the cholera-bacillus. He shows that there are several stages of growth of the cholera-bacillus.) The existence of the straight stage of the cholera-bacillus is of great importance to bear in mind, as it is probably the form which may of the bacilli assume in the intestinal contents in the early period of cholera, and as it would be impossible to recognise them as cholera-bacilli by the microscope alone, more especially when mixed with other bacteria. Hence, the discrepancy between the microscopic estimate and the estimate by cultivation, with regard to the numbers of these bacilli present in any given case, a discrepancy not remarked by me alone, but by several other observers. Indeed, from the very first, Dr. Koch found it impossible to distinguish by the microscope alone, in a mixture of bacteria, these bacilli from "other very similar forms of intestinal bacilli. If now we examine the specimens which have been in the incubator for a longer time (eight to ten hours) we find fewer small bacilli and a large number of larger distinctly curved forms, more especially at the edge of the drop; and in many instances, these are in pairs, forming the S-shaped form described by Dr. Koch. And there may also be a few spirillar forms, but I have not seen many in that stage. After this time development becomes less marked, and apparently soon ceases; whether from exhaustion of the nutriment or of the oxygen contained in the cell I cannot say, but I think most probably for the latter reason.

If a cultivation in the nutrient jelly be examined after two or three day's growth, at a temperature of 18° C. to 20° C., most of the forms will be seen to be markedly curved, though considerable variation exists, and some almost straight rods may often be found. From a very early stage of their growth in gelatin, they tend to group themselves together, to form little irregular zoöglœa-masses—the highly refracting particles seen in the cultivations with a low power, and likened by Koch to bits of glass; while in the fluid jelly (for they render the gelatin fluid), there are large numbers swimming about very actively. Now in these zoöglœa-masses, the bacilli are, as a rule, very distinctly curved. In those free in the fluid, the degree of curvature varies very much, some, as I have said, being almost straight. In the fluid, there will also be found S-shaped forms, consisting apparently of two organisms united end to end with the curves in opposite directions. In some cases, the union occurs with the curves in the same direction, as in the numeral 3. Longer spirillum-like forms may also be observed, likewise evidently composed of a row of comma-bacilli. If the cultivation be examined after five or six weeks, definite spirillar forms will be seen (how formed, whether from continued elongation and twisting of a single individual, or from fusion of the individual members of a chain, I cannot say). The spirals are uniform in thickness throughout, and do not show any trace of division.

Examined under a high power, (one twenty-fifth oil immersion lens), bacilli are very often seen, which do not stain equally throughout, but in which there may be two or three circular parts in which the stain is different in intensity from that in the rest of the rod. Examined on a dark ground, many of the rods are seen to be somewhat beaded, as is so often observed in other bacilli. It is not always possible to demonstrate this beaded appearance in every specimen of these bacilli, hence it probably depends partly on the amount of stain taken up, and partly on the stage of growth of the organism. Mr. E. M. Nelson also states that he has observed flagella in many specimens of this organism, generally one at each end.

The amount of curvature is, as far as I can judge, dependent to a great extent on the rapidity of growth of the organism. The more rapidly it grows, the shorter it is, and the less marked is the curvature. The more slowly it grows within certain limits, the more marked is the curvature, and the greater the number of S-shaped and spirillar forms. The most perfect specimen which I have seen, where the curve was marked in all the organisms, was obtained from a very slow and imperfect cultivation in jelly, which had not been neutralised, and which was distinctly acid. And I am told on good authority, though I have not yet had time to repeat the observation for

myself, that, if 5 per cent of alcohol be added to the nutrient jelly, the growth is slow, and the great majority of the forms observed are spirillar. I do not wish, however, to attach too much importance to the mere rapidity of growth as modifying the morphological characters of this organism, because I think other as yet undefined conditions also play an important part.

In glass-plate cultivations, the colonies are evident in about twenty-four hours, and appear, under a low power, as small, somewhat irregular pale masses. These gradually increase in size, and, where near the surface of the jelly, a small depression forms over them, so that, on looking from the side at the surface of such a cultivation, it presents numerous little depressions instead of the original smooth surface of the gelatin, each depression corresponding to a colony of these bacteria. As the colony increases in size, it becomes less compact, and the gelatine in the immediate vicinity becomes fluid. At this stage, the appearance is that of an irregular shaped mass of highly refracting granules, in the centre of a small area of fluid jelly, and floating about in the fluid are also other small refracting masses. When examined with a high power, these masses are seen to be aggregations (zoöglœa-masses) of comma-shaped bacilli, and the fluid is seen to contain large numbers of very actively moving bacilli. The character of the movement is very difficult to determine on account of its great rapidity, but the S-shaped and spirillar forms move in a distinctly corkscrew manner. I do not think that the single bacilli swim in this spiral manner, but of this I am not quite certain. When a bacillus is about to become incorporated in a zoogloea-mass, its motion is very characteristic. It moves backwards and forwards in part of the arc of a circle, the curvature being directed towards the centre of the circle, and away from the mass to which it is ultimately to be attached. After floating backwards and forwards in this way for some time—a movement which, I think, can only be accounted for by the presence of flagella—the range of movement becomes less and less, till the organism comes to rest at the edge of the mass. The colony goes on increasing in size for a few days, but ultimately ceases to extend or extends only very slowly. Dr. Koch reckons the ultimate extent of the colony at about one millimètre. This depends apparently on the amount of gelatin present, and is the result when ten per cent is employed; but apparently, if five or three per cent. gelatine be used, the colony may attain a considerably larger size than that mentioned. Something, I think, also depends on the amount of peptone added to the cultivating material. These appearances on glass-plate cultivations, taken as a whole, are, so far as I am aware, peculiar to the cholera-bacilli. I know no other organism which forms colonies on plates, which cannot be distinguished from those of the cholera-bacilli by a low power of the microscope.

The test-tube cultivations are also characteristic, but hardly so markedly as the glass-plate cultivations. In twenty-four hours, at a temperature of 18° Cent., growth is evident along the needle-track as a whitish line, broader at the upper part, and gradually tapering to the lower (the exact appearance depends of course on the size of the platinum wire employed; in all cases were a typical appearance is wanted, as thin a wire as possible should be used). At the upper part, the gelatine begins to evaporate, and there is slight depression. During the next twenty-four hours, the growth becomes more marked, and the depression increases in size so as to look like an air-bubble at the top of the track. In the following days the jelly at the top becomes liquid, and this liquidity extends gradually to the bottom of the track; thus there is a funnel-shaped appearance, from the greater amount of fluid at the top than at the bottom At same time, the mass of bacilli falls to the bottom of the fluid and assumes a somewhat rosy color, so that there is a rose-colored convoluted string running down the lower part of the track. The fluid at the upper part, which in about a week has extended to the sides of the test-tube, becomes clear, except a very thin layer at the top, which remains opalescent, the top itself being often covered with a very fine scum. Scattered over the solid gelatin forming the sides of the funnel are seen numerous small irregular highly refracting particles. These are the small zoöglœa-masses which have fallen to the sides and bottom of the funnel-shaped cavity. The rapidity with which the gelatine becomes liquid depends very much on the amount of gelatin, and possibly of peptone, present, as before remarked. In about three weeks, the jelly in the tube becomes entirely liquid, and then we have a clear fluid with a somewhat rosy mass at the bottom, a fine scum at the surface, and a narrow layer of opacity beneath it. In such a tube, the bacilli will be found alive after six or seven weeks from the date of inoculation. No one of these characters is of itself peculiar to the cholera-bacilli. They must all be looked at together, and the rapidity of growth must be taken into consideration as well. To my mind, the most typical appearance is that of the highly refracting particles lying on the side of the funnel, the liquid in the neighborhood being quite clear. The only other organism that I know which produces a somewhat similar appearance is Flügge's comma-bacilli, to be mentioned presently.

I have previously mentioned that the reaction of the material should be neutral, or slightly alkaline; and Dr. Koch did not think that this bacillus would grow at all in jelly which had not been neutralized, that is, which was distinctly acid in reaction. Not that he holds that all acids would prevent growth; for he points out that the surface of potatoes is acid, and yet these bacilli can be made to grow on them. I have, however, in two or three instances, obtained distinct growth of the cholera-bacilli in jelly which had not been neutralised, and which was found, when tested, to be distinctly acid. The growth was very slight; and, in fourteen days, there was only a thin whitish growth along the track of the needle (not more than is formed by the typhoid-bacillus in neutral jelly in the same time), and there was no liquefaction of the jelly, nor did the growth spread over the surface of the jelly.

(*To be continued.*)

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THE EDITOR of this JOURNAL would be glad to receive any items of general interest in regard to local events, or matters that it is desirable to call to the attention of the profession. Letters written for publication or containing items of information should be accompanied by the writer's full name and address, although not necessarily to be published. All communications in regard to editorial work should be addressed to the Editor.

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SATURDAY, JUNE 20, 1885.

THE STATUE OF BOUILLAUD.

On May 16 the statue of BOUILLAUD, member and president of the Academy of Medicine, professor and dean of the Faculté of Paris, and member of the Institute, was unveiled at his native place, Angoulême. Few memorials of scientific men have been more deserved than this; the work of Bouillaud extended over the greater part of a century, and in its variety and magnitude it was such as but few men are capable of performing. As is the case with so many of the best workers in science, he commenced life in great poverty, which though sometimes the evil sister of genius is often the mother of success and fortune. Though his medical studies were interrupted, like those of Broussais in 1792, by a call to the service of the army in 1815, they were resumed immediately on the completion of the campaign of Paris. He was graduated in medicine in 1823, and in the following year published the celebrated "Traité des Maladies du Cœur," by his master, Bertin. In 1825, at the age of twenty-nine years, he was elected to the Académie de Médecine; and in 1826, the year in which Laennec was lost to medicine, Bouillaud was elected Assistant Professor in the Faculty over a large number of candidates; and in 1831 he was appointed to the chair of clinical medicine. Thus, at the age of thirty-five, he occupied the chair which had been graced by Corvisart and Laennec.

His work, which hitherto had been great and laborious, now became stupendous; long hours of clinical work in the hospital and amphitheatres; active participation in the work of the Faculty; communications, reports and discussions in the Academy; laborious study in the works on ancient and modern medicine, and lectures on the works of the ancients and of the seventeenth and eighteenth centuries; experimental researches in the applications of physics and chemistry to medicine, thus in a measure fulfilling the prophecy of Bichat; writing of memoirs on physiology and medical philosophy; the publication of clinical treatises on Rheumatism and Diseases of the Heart, in which he showed himself the equal of Laennec and the superior of Corvisart. And it is gratifying to write that, with his reputation and knowledge, his fortune grew. Among his earliest works may be mentioned that in which he showed the part played by phlebitis and the obliteration of the veins in the mechanism of dropsies. To him also we are indebted for the explanation of the mechanism of albuminuria resulting from cantharidism, and of the slowness of the pulse in simple jaundice, which he showed was caused by the passage of the bile into the blood. His researches on the affections of the heart and on articular rheumatism led to a discovery which of itself is sufficient to render his name immortal, viz: the law of coincidence of inflammations of the heart with rheumatism. Until this discovery the causes of cardiac affections were absolutely unknown. Bouillard, however, threw great light on the subject, and he showed that, in the great majority of cases, these affections are produced by acute articular rheumatism, and that they are almost constant manifestations of this latter affection. The knowledge of this fact almost revolutionized the therapeutics of heart disease, and is justly considered one of the most important discoveries of this century.

Another memorable discovery of Bouillaud, is the influence of lesions of the anterior lobes of the brain on the function of language, of which he treated in his clinical researches on the subject in 1825; and although Broca, 36 years later, specified the seat of the lesions which determined aphasia, yet to Bouillaud is due the credit of making the first step towards the establishment of the doctrine of cerebral localisations. Besides his many monographs and treatises, he published numerous articles in the medical dictionaries and periodicals of the day.

France has been justly celebrated for the eloquence of her medical men; but none have been more eloquent than he, and none have ever infused into any language more of the delicate perfume of the ancient languages than did Bouillaud into the French. None are more aware of these facts than those who heard him at the International Congress held in Florence in 1865, or during the Exposition in Paris in 1867. The accredited orator of the Academy and of the Faculty of Paris, he made the orations at the unveil-

ing of the statues of Broussais, Bichat and Laennec; and could these masters have chosen one to set forth their works in their best colors, and who would at the same time do strict and impartial justice, they could not have chosen more wisely than did the Academy on these occasions.

It seems peculiarly fitting that the statue which has been raised to his memory should have been the work of an artist, M. Verlet, of his native place. And as it was fitting that Bouillaud should make the orations over the statues of Broussais, Bichat and Laennec, it was appropriate that his pupil Henri Roger should have in the same manner commemorated his works at the unveiling of his statue. In the closing words of his oration: Immortelle sera votre mémoire,

Nulla dies unquàm memori vos eximet ævo.

HÆMORRHAGIC MALARIAL FEVER; PATHOLOGICAL CHANGES IN THE BLOOD AND URINE.

DR. JEROME COCHRANE, of Alabama, in his paper on this subject published in this JOURNAL for May 30th, 1885, has given the results of the analysis of several specimens of the urine from patients laboring under this fever, in which the characteristically dark red urine, which has generally been supposed to contain an abundance of red-blood corpuscles, contained no blood corpuscles whatever. The color was due to the constituents of the blood-corpuscles, chiefly hæmoglobin, without the presence of any non-disintegrated corpuscles in the several specimens of urine examined. The microscopic examinations were made by Dr. Geo. M. Sternberg, U. S. A., and the chemical by Dr. H. Newell Martin, of the Johns Hopkins University. In the same paper Dr. Webb states, in connection with the cases related by him, that in 1875 he sent a specimen of apparently bloody urine from a severe case to Dr. James Tyson, of Philadelphia, for examination. Dr. Tyson reported that the specimen of "urine contained no blood-corpuscles," and that its color was due to hæmatin. But both Dr. Webb and Dr. Tyson then thought the absence of the corpuscles might be owing to their having been dissolved by the alkaline constituents of the urine after it had been voided. This supposition was disproved, however, by several of the specimens examined by Dr. Sternberg, in which he states positively that the urine gave an *acid* reaction.

All the important facts developed by the microscopic and chemical analyses reported in Dr. Cochrane's paper had been previously set forth very clearly in a short paper read to the Macon (Ga.) Medical Society by Dr. H. McHatton, September 2d, 1884, and published in the *Atlanta Medical and Surgical Journal* for October, 1884, pp. 400–1. Dr. McHatton, in giving the details of his examination, both microscopic and chemical, of the urine from a severe case of the malarial hæmaturia, says: "Although the urine was fresh, on repeated examination I could not find a single red or white blood-corpuscle." By chemical tests, he demonstrated the presence of abundance of hæmatin and hæmaglobin, and obtained "characteristic hæmin crystals." The latter he also obtained from a specimen of the matter vomited by the same patient. He adds: "This examination proved most conclusively that whereas the urine contained none of the anatomical elements of the blood, it was loaded with the chemical constituents. In fact it was a case of hæmaglobinuria and not hæmaturia, at the same time showing little or no renal troubles."

The important pathological inference from the facts adduced in the papers of Drs. McHatton and Cochrane is, that the blood in hæmorrhagic malarial fever has undergone such changes as a positive disintegration of its red corpuscles and diffusion of the hæmatine and other constituents into the serum of the blood, from which it is eliminated freely by the kidneys, and also deposited in the cutaneous and other tissues, imparting to them the characteristic hue usually accompanying that disease, there being no true renal hæmorrhage. On the other hand, many of those who have treated cases of this form of disease, state positively that the urine when voided contained undissolved blood-corpuscles. Thus the late Dr. S. M. Bemiss, of New Orleans, in the article on Malarial Fever in the first volume of the American System of Medicine, page 611, relates two cases of what he calls "malarial hæmorrhagic fever," in one of which he says, the "urine was loaded with albumen and showed under the microscope abundant blood-corpuscles;" and in the other that the urine was "loaded with blood." It is plain, therefore, that we need careful microscopic and chemical examinations of the urine immediately after having been voided, in a much larger number of well-authenticated cases of this variety of fever, before we can safely assume that all the essential pathological changes take place in the blood, and that the renal organs are only embarrassed by the effort to eliminate the products of the corpuscular degeneration of that fluid.

BURIED SUTURES.

In the *British Medical Journal*, of May 2, 1885, is a paper by MR. C. B. KEETLY, "On Buried Sutures, with Remarks on the Importance of Suturing Separately, Periosteum to Periosteum, Muscle to

Muscle, Deep Fascia to Deep Fascia, and Skin to Skin, after Deep Incisions of all Kinds." This is but the practical application in general surgery of the principles upon which fractured bones, divided nerves and tendons, wounds of the veins, intestines and other hollow structures are now treated. But, as Mr. Keetly remarks, these forms of buried sutures have a narrow, and, though a very important, a limited aim; the intestines, for example, being thus sutured in order to prevent extravasation of fæces.

The method now under consideration, however, is intended to "influence the whole course and final result of wounds in general. For instance, let us suppose buried sutures of the first kind to have been used to unite the two ends of a divided nerve; the use of the other kind of buried sutures would now commence, and proceed as follows: Whatever muscles or aponeuroses had been divided in cutting down upon the nerve would be restored to their original relationships, and kept there by aseptic animal sutures, such as carbolized catgut; then the wound in the deep fascia would be separately sewn up. Finally, the wound in the skin would be closed by either catgut or silver, or whatever might be preferred." The advantages claimed for this method are; that the necessity for drainage tubes is obviated. As no spaces or pockets are left in which blood or serum can collect, such collections do not occur; presuming, of course, that all blood-vessels have been carefully secured, and that the wound has been made thoroughly aseptic. The second advantage is that divided muscles and aponeuroses are so accurately sutured that perfect function is soon regained. The same is true of the deep fascia, which in some localities is of primary importance. *Thirdly*, deep, rough, and depressed cicatrices are avoided; and *fourthly*, necrosis of bone and sloughing of soft tissues are prevented.

In support of the practicability and advantages of his method, Mr. Keetly refers to seventeen operations, including two amputations of the thigh, and one of the leg, two excisions of the hip, one of *iridement* of the bones of the knee-joint, one wedge-osteotomy of the hip, one osteotomy of the tibia and fibula, two suturings of fractured patellæ, and one removal of sequestrum in necrosis of the symphysis pubis, with large abscess in the abdominal wall. In all of the seventeen cases, save two, "the buried sutures have done all which sanguine hopes could expect of them." He has also used this method with success in two cases of resection of the quadriceps extensor cruris, which operation, when performed for infantile paralysis with loose knees, Mr. Keetly does not think justifiable without the use of buried sutures. He especially recommends their use in operating on sebaceous cysts of the head. In concluding his paper, he says that it is only in strictly antiseptic surgery that he would venture to recommend the use of these sutures.

INJURIES OF THE MAIN BLOODVESSELS IN THE AXILLA CAUSED BY EFFORTS TO REDUCE DISLOCATIONS OF THE SHOULDER.

Such is the title of a paper by Dr. Lewis A. Stimson, read before the New York Surgical Society on May 26. The paper is based on a collection of forty-four cases, thirteen of which were fatal without operation, seven (all the cases) of double ligature fatal, six cases of recovery without operation, four cases of disarticulation at the shoulder with three deaths, and fourteen cases of ligature of the subclavian, with eight deaths and one unknown result. In commenting upon these cases Dr. Stimson thinks that it is a fair inference that conservative treatment may properly be tried at first, "but should not be prolonged if the symptoms do not promptly yield; and, secondly, that in case of resort to operation, ligature of the subclavian artery or disarticulation at the shoulder is to be preferred to incision of the sac and double ligature of the artery."

It is especially important to remember that in attempted reduction of these cases of dislocation any rough manipulation whatever should be avoided, and that abduction, circumduction, violent traction and forced pressure in the axilla are especially dangerous. It would seem that incision and liberation of the head of the bone, which has been tried in one case with success, is the preferable procedure in those cases in which conservative measures or gentle manipulation are of no avail. Injuries of the bloodvessels are more liable to occur in proportion to the time that the dislocation has lasted, as the adhesions become very firm. "Even a dislocated arm may be very useful, and the fatality of this accident, more than 70 per cent. of deaths, may well cause the surgeon to hesitate to incur the risk merely for the sake of ameliorating a condition which does not endanger life and is quite compatible with activity and usefulness."

TO SUBSCRIBERS, AND MEMBERS OF THE ASSOCIATION.

The next issue of the Journal will complete the fourth volume and the second year of the publication. That number will contain the official list of members in attendance upon the recent annual meeting of the American Medical Association, in New

Orleans, as furnished by the Permanent Secretary, a title page and a full index of the volume. As the number for the first week in July will commence a new volume and a new journal-year, it is a convenient time for new subscribers to commence their subscriptions, and for those who wish to become members of the Association to make their application to the Treasurer, Richard J. Dunglison, M. D., lock box 1274, Philadelphia, Pa. There are on our mailing list a considerable number of both subscribers and members of the Association, who have not yet paid for the past year. It is very desirable that all these should pay up without further delay. Subscribers should remit to this office, and members to the Treasurer, in Philadelphia.

SOCIETY PROCEEDINGS.

OHIO STATE MEDICAL SOCIETY.

(*Concluded from page 669.*)

THURSDAY, JUNE 4TH. SECOND DAY.

AFTERNOON SESSION.

DR. J. C. REEVE, of Dayton, delivered

THE PRESIDENT'S ADDRESS.

He spoke of the Evolution of the Science of Medicine. Though much that is imperfect still remains, medicine in the past or present compares favorably with law or theology. Medicine has always assumed the character of the age. For a long time it was bound to philosophy; but to-day it takes on the spirit of the age—active, practical, and advancing. He would have been a bold man who, at our meeting one year ago, would have told us that before our next meeting together we would have discarded ether and chloroform for diseases of the eye. There is an absence of any general theory of disease. Theories and hypotheses are the servants of the human mind. Systems are its masters. All systems owe their origin to some defect in the science of medicine at the time they began. They did a little good, but the greatest part passed into oblivion. Homœopathy, its dosage and incongruities, received their share of attention. Homœopathy has endured for three-quarters of a century. During that time the regular profession has discovered the stethoscope, ophthalmoscope, laryngoscope, ether, chloroform, chloral, cocaine, etc.

The following officers were elected:

OFFICERS FOR THE ENSUING YEAR.

President, W. Morrow Beach, of London; 1st Vice-President, H. C. Gill, of Cleveland; 2d Vice-President, H. R. Kelley, of Gallion; 3d Vice-President, Thomas Mackbreit, of Akron; 4th Vice-President, J. D. Roberson, of Wooster; Treasurer and Librarian, T. W. Jones, of Columbus; Secretary, G. A. Collamore, of Toledo; Assistant Secretary, E. C. Bush, of Zanesville.

DR. HIMES, of Cleveland, presented a very interesting essay on *Cases Connected with Life Insurance Examinations, with remarks.*

DR. G. W. RYAN, of Cincinnati, read a report on the

INFLUENCE OF SPINAL IRRITATION AS AN ORTHOPÆDIC FACTOR.

The influence of spinal irritation in the production of various deformities was carefully pointed out and numerous cases cited in proof of the author's statements. An interesting case of spasmodic clubfoot was related, which was readily relieved by treatment directed to the spine. Two other cases, who had been unable to walk for years because of this condition, were cured within a comparatively short time. The essayist placed little reliance upon the remedies ordinarily recommended. He had seen no good effect from the use of phosphorus, strychnia, or ergot. He had no experience in the use of opium. The proper and efficacious treatment is by means of blisters or the actual cautery applied along the spine. Spinal support is of benefit. Faradism and massage were valuable adjuvants to treatment.

Akron was chosen as the next place of meeting, to be held on the first Wednesday, Thursday or Friday in June, 1886.

DR. WM. T. CORLETT, of Cleveland, read a paper on

DISEASES OF THE SKIN OF REFLEX NERVOUS ORIGIN.

Two distinct varieties of nervous cutaneæ present themselves to the clinical observer. One is a senso-neurosis, the characteristic feature being a disturbance of cutaneous sensation unaccompainied by any visible lesion. Impressions made on the peripheral nerves are transferred ere they reach the *sensorium commune*, and are recognized as coming from parts more or less distant from their actual source. Transferred impressions may be found in other diseases than those of the skin. The surgeon looks to the hip-joint for the source of pain which the patient finds in his knee, and sounds for stone in the bladder when pain is complained of in the glans penis. The second variety is a tropho-neurosis, which is characterized by anomalies of nutrition. Impressions are transmitted more readily from cell to cell when the latter are in a state of exhaustion. Thus, a sudden or unexpected noise, or a flash of light, will call out motor and vaso-motor action.

Pruritus is a representative senso-neurosis, being a disease *sui generis* and not a mere accompaniment of cutaneous inflammation. After several years of study on this subject let me summarize as follows: Pruritus of the palms and soles is usually met with in females having some mild affection of the uterus of long standing, pruritus of the scalp in those suffering from subinvolution of the uterus or with a ruptured perineum. Pruritus *vulvæ* unaccompanied by a vaginal discharge is a common symptom of pregnancy. In males the most troublesome pruritus occurs in the genito-anal region. It is often difficult to diagnosticate this affection from eczema, on account of the delicate skin of the parts. When he first encountered this disease, the writer called it eczema. He does

not now so regard it because: 1, it seldom accompanies hæmorrhoids; 2, because it first appears as a pruritus; 3, because other anomalies of nerve influence at times accompany or alternate with that of sensation; 4, and because it is frequently limited to one-half of the body.

The most marked tropho-neuroses coming under the reader's observation have closely simulated eczema, but unlike eczema the eruption, whether papular, vesicular or squamous, remains as such throughout its entire course. Derangement of the sebaceous glands has been spoken of in this connection. Next to the eczematous group, it is most frequent.

Senso-neuroses are, *ceteris paribus*, more frequently associated with conditions of a mild nature, functional disturbances, nerve irritation and the like; whereas tropho-neuroses more frequently accompany pathological conditions of a graver import, *e. g.*, nerve lesions or gross destructive changes in the cerebro-spinal substance, although we must admit that the latter also are not infrequently due to functional disturbance alone.

Dr. Philip Zenner, of Cincinnati, Ohio, read a paper on

DISEASES OF THE SPINAL CORD.

In his opinion, errors in diagnosis are less commonly due to real obscurity about a case than to an absence of knowledge of the leading factors upon which a diagnosis is based. In proof of the truth of this statement he selected a number of cases, representing the most common diseases of the cord, from his case book, in which errors of diagnosis had been made, even by men of prominence.

Case 1.—Mr. W., age 38, had been treated for rheumatic pains. When first seen he gave a history of having had frequent paroxysms of severe lancinating pains in the lower extremities for several years, and also various anomalous sensations, numbness and formication in the limbs, a sense of constriction about the waist, etc. On careful examination some impairment of sensation in the lower extremities, a degree of insecurity in the feet when the eyes were closed, and an absence of the patellar tendon reflexes were found. There was also already observable some impairment in the gait, which, the patient stated, was worse at night. The symptoms in this case were so pronounced that there was no excuse for an error in diagnosis. Locomotor ataxia should have been positively diagnosed. This diagnosis is based upon the history of characteristic lightning pains, peculiar sensations, and difficulty of walking at night, together with the slight anæsthesia, inability to stand firmly with closed eyes, and absence of the patellar tendon reflex. Yet cases occur in which a diagnosis can not be made so easily, where the nature of the pains is more doubtful, as the disease has made but little progress and its more striking symptoms are, therefore, not yet manifest.

He wished to lay especial stress upon a few symptoms which, even at an early period, enable us to make a positive diagnosis. These are the characteristic lightning pains, the absence of the patellar tendon reflex, and certain ocular symptoms which, though not found in the patient just spoken of, are often the earliest manifestations of the disease. These symptoms are: transitory paralysis of some of the external muscles of the eye, causing double vision; the so-called Argyl-Robinson phenomenon, a small pupil, which does not respond to light, but contracts during the act of accommodation; and, more rarely, atrophy of the optic nerves. To the early symptoms may be added one other, the Brach-Romberg symptom, inability to stand firmly with closed eyes, though perhaps too much importance has been attached to it.

Case 2.—Mr. K., age 35, has had some difficulty in walking for about six years. He has also had slight bladder symptoms. Locomotor ataxia had been diagnosticated, but the conditions present were quite different from those of locomotor ataxia; the gait was feeble, not ataxic. There was often a feeling of stiffness in the limbs. The patellar tendon reflex was exaggerated, and, another manifestation of excessive tendon reflexes, the fool clonus could be elicited. The three symptoms upon which the writer laid so much stress in connection with locomotor ataxia, lightning pains, absence of patellar tendon reflex, and ocular symptoms, were not present. Another symptom, swaying of the body when trying to stand erect with closed eyes, was also present, though too much reliance must not be placed on this symptom, as it is often misleading.

This patient had a different disease of the cord. In locomotor ataxia the posterior columns are the chief seat of disease; in this patient there were changes in the antero-lateral columns, what is usually termed laterial sclerosis. Its usual symptoms are altogether motor, paralysis with oftimes rigidity or spasmodic contraction of muscles, and exaggerated tendon reflexes.

It may appear to the superficial observer that there is little practical difference whether the disease is limited to the posterior or lateral columns, that a nice diagnosis is of no consequence to the patient. But, in fact, it is a matter of much consequence. Not only are the subjective symptoms of locomotor ataxia severe, while those of lateral sclerosis are insignificant, but the prognosis of the latter is decidedly better. In illustration of the latter statement he mentioned a case seen in Newport in consultation with another physician. When he first saw the patient, about nine months ago, he had been unable to make the least voluntary movement of the lower extremities for over a year; both the family and the physician in attendance believed the case to be utterly hopeless. At the present time the patient can move his limbs freely, and there is some hope of a complete recovery.

Case 3.—Mrs. N., age 30, four years previous to my seeing her, observed some weakness in the lower extremities, so that she was unable to lift the feet in walking as well as formerly, could not arise easily from a sitting to a standing position, etc. The weakness subsequently invaded the muscles of the trunk and the upper extremities. When the writer saw her, the deltoids, the biceps of the arms, and the quadriceps femoris of both sides, as well as many of the muscles of the back were almost completely paral-

yzed. Especially noticeable was the patients method of arising from a sitting to an erect position. She succeeded in extending the knees by pressing with the hands upon the thighs, and then raised the trunk with the assistance of a chair or other external support In this effort a remarkable curving of the spine in the lumbar region was produced, presenting an appearance as though there were no unyielding parts about the spinal column, and led to the suspicion on the part of one of the physicians who saw her, that she had disease of the vertebræ.

But a diagnosis in this case ought to have been readily made. There was in addition to the paralysis, atrophy of the muscles, and, a point upon which too much stress can not be laid in connection with the diagnosis of such cases, a loss of electrical contractility in the affected muscles. The atrophy and altered electrical reaction of the muscles, together with the history of gradually extending paralysis, were quite sufficient to establish the diagnosis of progressive muscular atrophy, though in this case, the manner of development of the disease was an unusual one, and had, therefore, misled other observers.

CASE 4.—Mrs. L., age 52, had been operated upon for carcinoma of the mammary gland. About eight months after the breast was removed, she began to suffer with severe pain on the same side, like that of intercostal neuralgia. Subsequently she was seized with severe pain in the lower extremities, hyperesthesia of the surface, and paraplegia, with some atrophy of the muscles of the legs. Dr. Zenner saw her in consultation about six months after these symptoms were first manifested. The symptoms were those just given. Myelitis had been diagnosticated. He called attention to three important factors, the gradual access of the symptoms; the severity of the pains, seldom, if ever, found in ordinary myelitis; and the history of a previous malignant disease of the breast; and gave, based upon these factors, a positive diagnosis of malignant disease within the vertebral canal, and a prognosis of a fatal issue within a year. The subsequent history of the case strengthened the diagnosis. There was a gradual increase in the paralysis and muscular atrophy, motion of the vertebral canal became very painful, and, at a later period, there occurred some deformity of the spine. The patient died nine months subsequent to his seeing her, and, on *post-mortem* examination, numerous carcinomatous nodules were found in the vertebræ and the chord. The symptoms which became manifest after he saw the patient, pain on movement of the spine and deformity, indicated that the vertebræ were the seat of disease, and were, therefore, of great weight in diagnosis. But the earlier manifestations were quite sufficient to make a correct diagnosis. Paraplegia, with pains so severe that the name paraplegia dolorosa has been applied to designate it, is rarely, if ever, found excepting with malignant disease. When we find, in addition, that malignant disease has been found in another part of the body, no doubt of the diagnosis should be entertained.

If a patient comes with a history of pains, of weakness, or anomalous sensations pointing to the chord, we should examine as to the presence of ocular symptoms, as to the condition of the cutaneous sensibility of the tendon reflexes of the vegetative functions, especially bladder and rectum, and as to the kind of gait, to see whether there are any indications of the presence of locomotor ataxia. We should also examine to see whether any motor paralysis with increased tendon reflexes and rigidity of muscles point to disease of the motor columns of the chord, or paralysis and atrophy of muscles with altered electrical reaction point to affection of the gray matter. If all such symptoms can be excluded, we are probably in the presence of a functional disease, which, with time and proper treatment can be entirely removed, and whose positive diagnosis will bring immediate mental relief to an anxious, perhaps despondent, sufferer.

FRIDAY, JUNE 5, THIRD DAY.

DR. N. P. DANDRIDGE, of Cincinnati, read a paper on

OSTEOTOMY FOR DEFORMITIES.

He reported the case of a patient with a malformed hip, the femur being much distorted. He cut through the deformed tissues of the thigh and placed the parts in their natural position, confining them by means of bandages. The patient in a few months regained the use of the formerly deformed limb, and was able to walk in the natural position. Other cases were also reported.

DR. H. G. LANDIS, of Columbus, read a paper on *Occipito-Posterior Positions;* Dr C. H. Von Klein, of Dayton, a paper on *The Voice in Singers;* Dr. Joseph Ransohoff, of Cincinnati, read a volunteer paper on *Urethral Calculus;* and Dr. H. J. Herrick, of Cleveland, a volunteer paper on *Dietetics in Typhoid Fever.*

DR. W. MORROW BEACH, of London, was introduced and made an address.

Resolutions of thanks for receptions at the Southern Ohio Insane Asylum, the National Soldier's Home, and one by the Montgomery County Medical Society, were passed.

The Society then adjourned.

GYNÆCOLOGICAL SOCIETY OF BOSTON.

Annual Meeting, January, 1885.

The President, H. O. MARCY, M. D., in the Chair.

The paper of the meeting was the Annual Address of the President on

THE RÔLE OF BACTERIA IN PARTURITION.

About five years ago Dr. Marcy read a paper upon "Antiseptics in Midwifery," in which he taught that instrumental deliveries should be conducted with all the care and precaution of a major surgical operation, and that antiseptics were of equal importance in both. Since then, not only have these views been generally accepted, but it is now taught at Harvard that the antiseptic vaginal douche should be used in *all* cases of normal labor.

In normal labor the uterine cavity probably remains aseptic, and its denuded surfaces and lacerations are not unlike subcutaneous wounds; therefore the lochial secretion remains a non-irritant, innocuous

fluid. This secretion is highly albuminous, and is a perfect fluid for the rapid propagation of micro-organisms accidentally introduced, but not normally present, in this perfect incubating chamber. Some writers assert that the bacteria normally present in the vagina act as causative agents in the production of puerperal fever; and, on this ground, advise antiseptic vaginal injections before labor. This is not the case. The bacteria found in the vagina are the bacteria of decomposition, and do not develop readily in the blood, and when injected act as chemical irritants. If they were the offending germs all primal labors would develop symptoms of septicæmia, for in these cases there are always solutions of continuity, either cervical, vaginal, or perineal. This bacterium of decomposition is rarely a source of danger, but may serve a useful purpose by utilizing and exhausting the material which might feed an army of more dangerous enemies. They are rarely if ever found in the uterine cavity, as the lochial outflow is constant during the first few days and prevents their progress toward the uterus. It is evident that solutions of continuity are not of themselves sources of danger, since it is not shown that primal labors are more frequently followed by septicæmia than others.

When manipulative interference becomes necessary the whole phase of the question changes, for we are now incurring danger of introducing germs from without, and strict antiseptic precautions become necessary. The hands of the operator should be aseptic, the uterine cavity thoroughly cleaned by antiseptic applications, and firm contraction of the uterus maintained by ergot and compression. The same general rules govern here as in surgery, for puerperal fever and surgical septicæmia are the same. The microörganism that is the cause of puerperal fever is the round-cell growth, the micrococcus, which develops with wonderful rapidity in the blood The planting ground is rarely the vagina but usually the uterus. Its development is not accompanied by any odor. When these micrococci have gained admission and infection has taken place, intrauterine douches cannot undo the mischief, as the germs have already passed into the blood and tissues, and are there rapidly reproduced. Our injections may cut off reinforcements but cannot stay the progress of the army already advanced beyond the point of attack. It may be best to treat the uterine cavity as an infected, suppurating, and sloughing wound, by using the curette or applying pure carbolic acid to prevent further absorption and stimulate the surrounding tissues.

The relation of erysipelas to puerperal fever was first advocated in Boston. Within a year five fatal cases have occurred in the practice of one physician following a case of erysipelas. Sterilized bulbs inoculated with fluid taken from just within the *os uteri* produced a form of micrococcus which could not be differentiated from those taken from the blisters of erysipelas.

Too much importance has, undoubtedly, been placed upon injections. In normal labor they are unnecessary and may be dangerous.

Dr. W. S. Brown was inclined to be skeptical in regard to the causal relation of microörganisms to the troubles of the lying-in woman. He thought the tube of the syringe might do harm and cause hæmorrhage. He never used the douche unless he had reason to believe there was some noxious material to wash out. He found that filling the spaces under the finger nails with softened soap a convenient and efficient means of preventing infection from that source.

Dr. Horace White had had no case of puerperal fever in 25 years of practice. He did not use douche unless it was specially indicated. Aside from thorough cleanliness he had adopted no Listerian precautions.

Dr. Grainger had not used antiseptics for 15 years, even in cases of forceps or turning. He thought septicæmia likely to follow post-partum hæmorrhage, and always gave ergot and quinine in such cases. He had used intrauterine injections only three times in his practice.

The

ANNUAL ELECTION OF OFFICERS

resulted as follows:

Henry O'Marcy, M.D., President.
A. L. Norris, M.D., Vice-President.
H. J. Harriman, M.D., Secretary.
W. S. Brown, M.D., Treasurer.

The Pathological Committee, as appointed by the Chair, consists of Drs. A. P. Clarke, E. W. Cushing, and W. O. Hunt.

OBSTETRICAL SOCIETY OF PHILADELPHIA.

Stated Meeting, June 4, 1885.

The President, B. F. Baer, M.D., in the Chair.

Dr. E. P. Bernardy, read a paper on

BINIODIDE OF MERCURY AS A DISINFECTANT IN OBSTETRICS.

His attention was first attracted to the use of bin-iodide of mercury as a germicide by Dr. Miquel, who published in *L' Annuaire Meterologique de Montsouris* the results of some experiments made to determine the minimum amount of a disinfectant necessary to prevent fermentation in a litre of sterilized beef-broth. His experiments show that the mercurials are the best antiseptics, the biniodide being nearly three times as strong as the bichloride. In his table of disinfectants he places the bichloride the fourth on the list. To a litre of sterilized beef-broth, he found it required 0,025 grammes of the biniodide of mercury to keep the broth pure; while 0,070 grammes of the bichloride of mercury was necessary to produce like effects. This shows that bacterial life is impossible in a solution of one forty-thousandth part of the biniodide, while of the bichloride it requires the one fourteenth-thousandth part. I was so favorably impressed with his experiments that I determined to give the biniodide of mercury a trial in obstetric cases in which it would be necessary to use an antiseptic. The following are the cases in which it was used.

Case 1.—On February 7, '85, I was requested to take charge of Mrs. D., who had been confined about six weeks previously. It had been her second confinement, the duration of labor had been short and delivery natural, but an extensive laceration of the perineum had occurred. No attempt had been made to bring the parts together by sutures. On the third evening she had been attacked with severe frontal headache and chills followed by fever, with great tenderness on the region of the uterus. There being no improvement in her condition, her medical attendant was discharged, another called in who gave such an unfavorable prognosis that he also was requested to cease his attendance. I was finally called in on the above date. The patient had well-marked symptoms of septic poisoning, pulse 130 to 140, small and thready, and disappearing under pressure of the finger; temperature 104—105°; slightly delirious; constant vomiting; abdomen swollen and excessively tender; uterus enlarged, extending fully three inches above the pubio. In the right side there seemed to be a growth extending up into the abdomen, tender on pressure. On making a vaginal examination I found the os dilated so that my index finger could readily enter the uterus. Its withdrawal was followed by a gush of highly offensive matter. The uterus was surrounded by organized lymph and was immovable. The mass on the right side was easily detected and was continuous with the lymph surrounding the uterus. The vagina was hot. The perinæum was torn to the anus; the surface raw and secreting an acrid matter which scalded the surrounding parts. The urine was dark; on standing a reddish material settled to the bottom, it looked like blood corpuscles. Dr. A. E. Rowsel examined the specimen and reported it to be slightly acid, no albumin nor sugar; under the microscope occasional pus corpuscles, and swarming with bacteria. In conjunction with internal treatment, intra-uterine injections were made three to four times a day. I first used a solution of bichloride of mercury, one to two thousand. This was continued for three days without any marked results. The discharges continued as offensive. On the fourth day the bichloride was replaced by a one to four thousand solution of biniodide of mercury. Within twenty-four hours an amelioration of all symptoms took place, the pulse fell to 100, temperature 101°. Urine became clear, and the discharge odorless. The injections were continued for ten days, their frequency being gradually reduced. The uterus returned to almost its normal size and the lymph was gradually absorbed. The patient recovered.

Case 2.—March 19, '85, I was called to attend Mrs. H. in her first confinement. On my arrival found she had been in labor several hours, examination showed the os perfectly dilated, bag of water protruding, vortex presentation, first position, ruptured the amnion. The vagina near its outlet was roughened with venereal worts, these spread also over the vulva. Labor progressed rapidly and the second stage was happily ended. After waiting nearly an hour making compression on the uterus, I made slight traction on the cord, and while doing so, felt, with my hand upon the uterus, a cup-like depression of the fundus take place. This convinced me that I had an adherent placenta to deal with, and it would be folly to wait any longer; on introduction of the hand I found the placenta completely adherent, one could hardly say which was uterus, which placenta. After considerable trouble, I at last succeeded in detaching the placenta: it took fully three-quarters of an hour. On making a second examination to ascertain if all had been removed, my hand came in contact with long shreds hanging from all sides of the uterus; the more I scraped, the more there seemed to be. I gave the patient B. ij. of the extract of ergot. The patient did well for ten days, when toward evening she complained of a chill and severe frontal headache. I gave her quinine sulph. gr. x., with morph. sulph. gr. ¼ at one dose and washed out the uterus with 1 to 4,000 solution of biniodide of mercury. The pulse was 115, and the temperature 102°. The discharge was highly offensive. The injections were repeated every four hours. On the evening of the next day the pulse was 98, temperature 100°, discharge odorless, and the patient was perfectly well in ten days more.

Case 3.—April 23, '85. I was requested to call at once to see Mrs. K., who was reported in eminent danger of death. This was her ninth confinement. The history of the previous ones, with one exception, was not good. Her labors were natural, but were followed by terrible flooding and protracted convalescence. I found the patient in an attack of puerperal convulsions. I gave at once grs. xxx of potass bromide, and grs. xx of chloral hydrate; this dose was repeated in half an hour; ten minutes later another convulsion occurred. I then bled the patient freely. The os-uteri was somewhat enlarged, the cervix soft and dilatable; vertex presentation. Dr. Custin, whom I had sent for, having confirmed my opinion, and considering that the patient's time was quite up, we decided to etherize, dilate the cervix and deliver. The forceps were applied, and traction made at intervals. A living child was safely delivered. Continuous pressure was maintained over the uterus, but after the expulsion of the placenter the uterus did not contract until it had been washed out with hot water. The bromide and chloral was continued every two hours and no more convulsions occurred. The patient did well up to the fourth day when the discharges became very offensive, the pulse accelerated, and slight tenderness existed over the uterus; no chill or fever. The uterus was thoroughly washed out with a solution of one to four thousand biniodide of mercury. Within twenty-four hours the discharge became odorless, and the tenderness over the uterus had disappeared. The patient recovered after a tedious convalescence.

In these cases it will be seen that the biniodide was prompt in its action; markedly so in case 1, where the bichloride and the biniodide were both employed, the result being decidedly in favor of the biniodide. Naturally, it will be said, here are only

three cases from which deductions are to be drawn, and it is only after it has been carefully used in a large number of cases that its efficacy can be proved. It is for this reason that I call the attention of the members of this society who are in a position to give it a fair and impartial trial, and at some future time give the results of their investigations. I have found the 1 to 4,000 solution of the biniodide non-irritating. I have used it extensively in my gynæcological practice, and in washing out pus cavities, with good results. In it we have a preparation where the smallest amount of drug is used with results far exceeding those of any other antiseptic. On account of the small quantity of mercury, there will be less chance of salivation. The method I have pursued in making the solution is: Take three and a half grains of the salt well triturated in a mortar and rubbed, with one quart of boiling water slowly added, giving a solution of 1 to 4,390. Since writing the above, I have seen in the *Philadelphia Medical Times*, May 16, 1885, that Dr. Panas, eye-surgeon of the Hôtel-Dieu, uses the 1 to 25,000 solution of biniodide of mercury in eye cases. He makes the following statement: "After a number of experiments, I have convinced myself that a solution in water, 1 to 10,000, of the bichloride, or a similar solution, 1 to 25,000, of the biniodide of mercury, is much superior to any other antiseptic solution employed in eye-surgery." Here again we have a statement that the biniodide in a smaller quantity is as good an antiseptic as the bichloride.

Dr. Montgomery said that his experience has led him to the conclusion that the bichloride of mercury is far more effective as a disinfectant than carbolic acid. Its introduction into the Philadelphia Hospital was due to Dr. Parvin, who found it very satisfactory. In eighteen cases of puerperal fever that were treated with bichloride injections, only three deaths occurred. This success was attributed by the Hospital staff to the use of the bichloride. Dr. Montgomery, in private practice, follows the plan of Dr. Garrigues, of New York, and avoiding intro-uterine injections after labor, but applies pledgets wet with a bichloride solution over the vulva, after carefully cleansing away clots and washing the external parts with a similar solution; this sponging is repeated twice every day. In his last term at the Philadelphia Hospital he had only two cases of puerperal fever, one of which commenced twelve days after delivery. The history of Dr. Bernardy's cases shows an equal, if not better, antiseptic in the biniodide of mercury.

Dr. J. V. Kelly, some years ago, had several fatal cases of puerperal fever, and about twenty-five cases that got well. The trouble commenced in a case of abortion in which he did not succeed in removing all of the placenta. He was at the same time attending a bad case of erysipelas, and at that time the relationship of puerperal fever and erysipelas was not known to him. He was on the point of giving up his practice and leaving the town, and he consulted Dr. Goodell on that question. Dr. Goodell discountenanced such action, but advised him, when attending an obstetric case, to remove his coat and roll up his sleeves, and wash his hands and arms well with turpentine, using the nail-brush thoroughly. Since that time he washes his hands in turpentine every day, and again before every case of labor. He also uses a wash of vinegar or carbolic solution before touching a puerperal patient. He has had no puerperal fever or other septicæmic symptoms since that time.

Dr. Parvin said that, as a reference has been made by Dr. Montgomery to my having used corrosive sublimate vaginal injections in the cases of puerperal septicæmic under my care in the Philadelphia Hospital, in my term of service last year, I will refer to the antiseptic treatment in the cases occurring the present year. When I took charge of the obstetric ward, on the first of January, I found five recent cases of septicæmia; two of these patients died; one of the two had apparently recovered, and then was attacked by pneumonia. Quite possibly this pneumonia had a septic origin. Then there were at least seven other cases, but all these, as well as three of the original five, recovered. Injections of a solution of corrosive sublimate, 1 to 5,000, were used in all cases immediately after labor. The external parts were washed, too, with a similar solution. This injection was repeated twice a day in all cases for the first week after labor. While it was used oftener in those having septicæmics, intro-uterine injections were used only when surgical injections failed to correct the offensiveness of the discharge; but as is well known, there may be serious, even fatal cases of septic disease, though the lochial flow is not at all offensive. In private practice, after once washing out the vagina thoroughly with the antiseptic solution, immediately after labor, this need not be repeated unless symptoms demand it; but the bathing of the vulva twice a day with the solution ought not to be omitted. Add, if you please to this treatment, the use of antiseptic napkins, a practice pursued by Dr. Montgomery, of the Philadelphia Hospital, so successfully, and I think we have taken the most important means to guard against the entrance of septic germs after labor. He has had no experience with the biniodide of mercury, and does not know that it will supersede the bichloride. The argument in its favor is as strong as those successful cases can make it, but these are entirely too few, as Dr. Bernardy justly says, to prove its value and its superiority. In one of his cases labor was induced apparently on account of eclampsia. Now, is this the best treatment? Obstetricians are by no means agreed, some of the best condemning such treatment. But the subject is not properly before us now, and therefore no further remarks will be made upon it.

Dr. Montgomery uses the bichloride solution as an external wash only; not as an injection. He thinks the records of the hospitals in which injections are used will not show as good results as those in which they are omitted, if septicæmia be not present.

Dr. Willard had a warning to sound with regard to the use of bichloride solutions of stronger grades. He had been using washes and antiseptic dressings made with a 1 to 1000 solution of bichloride, but in consequence of what was written about the advan-

tages of stronger solutions, he increased the strength of his dressings to 1 to 500, and within twenty-four hours the stools contained bloody mucus and were small and griping; there was vesication about the wound and around the limb under the dressings. Entirely dry dressings had been used, but they had been moistened by pus and serum from the wound. He does not see the advantage of strong solutions in serum as one to 100 or 75. The serum is a decomposable substance, and an uncertain portion of the antiseptic agent is destroyed by it. Weak solutions in boiled water seem more reasonable, and answer every purpose.

DR. LONGAKER has been much interested in the third stage of labor, and would like to hear how Dr. Bernardy removed the placenta in the adherent case narrated by him. He has been using Credé's method with great satisfaction. He thinks the hand should be kept out of the parturient canal as much as possible. He does not need vaginal injections after labor, but depends upon outside washes. He finds that the temperature rarely rises during the puerperal period, even after instrumental delivery. He thinks care during the third stage will avoid the necessity for antiseptics.

DR. PARISH agrees with Dr. Parvin's views. He has found a solution of 1 to 1000 of bichloride irritating, and he now uses 1 to 2000 or 5000. Strong solutions cause an appearance resembling erysipelas or inflammation of the derm. Injections are not necessary in every case; when the surroundings are cleanly and the patient a multipara he does not use them, but in primiparæ with contusions or lacerations, and when version or instruments have been employed, he is in the habit of injecting a weak solution of bichloride immediately after delivery, but he does not repeat it, but simply washes out the vagina. Cleanliness of hands, instruments and nurse is the most important point. He has never had any trouble in private practice.

DR. BERNARDY, in closing the discussion, remarked that he thought he had a far better antiseptic in biniodide than the bichloride. He had used the former in surgical cases also, washing out pus-cavities, and always with good results. He does not use intra-uterine washes in every case of labor. He has attended since the first of the year about seventy cases of labor, and he has employed the intra-uterine injections in only the three cases detailed. In every case of labor he uses carbolic acid soap to cleanse his hands and arms and the external genitalia of the patient. The eclamptic patient had reached or passed her full term, and there was no reason why the child should not be removed. In the case of adherent placenta, it was pulled off forcibly, and a shreddy lining was left in the uterus, as no line of separation had formed. He believes the dangers consequent on passing the hand and arm into the uterus and vagina are much exaggerated, and he does not hesitate to do so when the exigencies of the case demand it.

(*To be continued.*)

STATE MEDICINE.

METHOD OF GRANTING THE LICENSE TO PRACTICE MEDICINE.

The following report was made to the Medical Society of the State of Pennsylvania at its recent annual meeting, by a committee appointed a year ago to consider and report upon this subject:

By enacting the registration law of 1881, Pennsylvania has declared that the practice of medicine should be subject to legal restriction. And in all parts of the United States, the tendency seems to be towards the establishment and further elaboration of some system of State license, to control entrance upon the practice of medicine.

The only sufficient reason for the existence of any such system is that the license granted shall be reliable evidence of the fitness of its recipient for his work. A system which secures this may be defended —no other can. If a State license helps the community to the choice of competent medical advisers, it at least accomplishes that for which it was instituted. But if it be conferred upon the incompetent as well as upon the competent, it serves to confuse and mislead rather than to guide the people to a wise choice.

If, then, we are to have a State license, it should possess a real, fixed and easily ascertainable significance. The license granted under the present system does not conform to this condition. It is practically the diploma of any "legally chartered medical college or university having authority to confer the degree of Doctor of Medicine." Being granted by any one of over one hundred competing and interested corporations, upon evidence of fitness furnished by a private unrecorded examination, its significance is neither fixed nor ascertainable, and therefore is not real.

But however uncertain it may be as to its antecedents, the State license does and must confer upon every recipient precisely the same privileges and immunities. Hence the competition in granting it cannot be upward. One corporation cannot put itself ahead of another by granting a better license. But it may outstrip its rival by competing downward and bestowing the license more cheaply by requiring of the recipient a lower grade of fitness.

The medical student may seek the school offering the best teaching, the greatest laboratory and clinical facilities; but he does not seek the school which will compel him to pass the strictest examination before granting him the medical decree. Generally he rather avoids that school. For its diploma confers no greater legal privilege, and, to the mass of laymen carries no greater scholastic prestige than the diploma of its less censorious, less conscientious rival. And he can have no desire to run even a fancied risk of missing the degree, merely for the sake of a harder examination, without any concomitant advantage.

Hence the temptation is continually presented to the faculty of every medical school to endeavor to enlarge its classes by reducing or keeping low its standard of requirements for graduation. Nay, it is

more than a temptation. In competing for the patronage of students, under the present conditions of public opinion and statute law, a low standard of requirements is an essential to success. Because of this downward competition it entails, the power to grant the State license should no longer be vested in medical schools.

By general experience publicity has been proven the best shield against laxity of administration and the injustices of favoritism and prejudice. In matters of this kind publicity is more than a shield against such abuses. It is the destruction of the conditions necessary to their existence. And it should be allowed to guard the interests of the community and the profession in the granting of the State license to practice medicine. The present method of secret and unrecorded examination is the best that could well be devised to foster abuses of the licensing power. How can this be changed? Under their charters medical schools have certain vested rights, which the courts would protect against legislative abridgement. Among these is the right to determine how and by whom the fitness of candidates for their degrees shall be ascertained. Even if some medical schools would submit to some desirable changes in their methods of examination; while their action would place them at a disadvantage in the competition with other schools unhampered by any such restrictions; a large proportion of the State licenses (all granted by these unrestricted schools) would remain as unreliable and deceptive as ever. The colleges cannot be restricted in the granting of their degrees. But the degree of Doctor of Medicine was not primarily a license to practice. It was made a license by the act of 1881. That act can be repealed, or so amended as to transfer to other hands the licensing power. And, here, we believe lies the remedy for those abuses which medical teachers, practitioners and clients should equally deplore: To prevent downward competition in the granting of State license; to relieve medical schools of the degrading influence such competition exerts; to regulate the licensing of those obtaining the medical degree from institutions outside the State; to guard by publicity and direct control against the abuse of the licensing power, and to ensure the efficient administration of laws looking to these ends; we recommend that this Society endorse and advocate before the people of Pennsylvania and their representatives in the State Legislature the following propositions:

First—The sole power of granting the license to practice medicine in Pennsylvania should be vested in a single board.

Second—The board should have no connection with any corporation engaged in medical teaching.

Third—The doings of this board, including the determination of the fitness of applicants for the license to practice, should be public and matters of record, and the records open to public inspection.

Fourth—To secure the efficiency of the board against incompetent membership and the undue pressure of partisan politics, the medical profession or its representatives should nominate to the Governor a list of persons qualified to perform its duties, from which list the board should be appointed.

It is also recommended that a committee be appointed to prepare, in accordance with the views of this society, a form of bill suitable for presentation to the next biennial session of the Legislature, and to report the same to our next annual meeting.

TRAILL GREEN, FRANK WOODBURY,
WM. VARIAN, LOWELL M. GATE,
JOHN M. DEAVER, EDWARD JACKSON,
HENRY LEFFMAN,

The society accepted the report and proceeded to consider its recommendation *seriatim*. After considerable debate, and the rejection of various amendments, they were, by a large majority, adopted.

FOREIGN CORRESPONDENCE.

LETTER FROM PARIS.

(FROM OUR OWN CORRESPONDENT.)

The Bouillaud Statue—The Prizes of the Academy of Medicine—Death of Dr. Gueneau de Mussy.

A statue to perpetuate the memory of the late Professor Bouillaud was erected by public subscription at Angoulême, the town nearest his birth place, which is a village in the department of the Charente. The statue was inaugurated on the 16th inst., in the presence of the officials of the town, and of several members of the profession from different parts of the country, as well as of representatives from the various learned societies to which Dr. Bouillaud belonged. Among the medical men from Paris, I may mention the names of Professor Cornil, Potain and Verneuil, also Dr. Léon Labbé, Hospital Surgeon, who were all pupils of the celebrated physician. After a few words of welcome from the Mayor of the town, the delegates addressed the assembly in turn, Professor Laboulbène in the name of the Faculty of Medicine of Paris, Professor Vulpian for the Academy of Sciences, and Dr. Roger for the Academy of Medicine.

The Paris Academy of Medicine distributed its annual prizes at its meeting on the 19th of May. The following is a list of the principal recipients and the subjects on which they treated: A prize of 2,000 francs was awarded to Dr. Paul Richer for his work "On Hysterical Paralysis and Muscular Contractions:" 1,500 francs to Dr. Pierre Marie, for his memoir "On Sclerosis Disseminated in Patches;" 500 francs to Dr. William Murrell, of the Westminister Hospital of London, for his work on "Nitro-glycerine as a remedy for Angina Pectoris;" 500 francs to Dr. Henri Huchard, of Paris, for his work on "Angina Pectoris;" and another sum of 500 francs was awarded to the same physician for his works on other subjects; the prize of 1,000 francs was divided between Drs. Josias and Nocard for their memoir entitled: "Experimental and Clinical Researches on the Treatment of Itch and of the Acarus Scabiei by Naphthol;" 500 francs with honorable mention to Dr. Eugène Rochard for his work "On the Employment of Min-

eral Waters in Surgical Affections;" a special prize was awarded to Dr. Javal for his memoir "On Ophthalmometry;" the sum of 2,000 francs was divided between Professor Arloing, of Lyons, and Dr. E. P. Guiard, of Paris, the former receiving 1,200 francs for his "Experimental and Comparative Researches on the Action of Chloral, of Chloroform, and of Ether;" the latter 500 francs for his "Experimental Clinical Studies on the Ammoniacal Transformation of the Urine." A prize of 3,000 francs was awarded to Professor Denuce, of Bordeaux, for his "Clinical Treatise on Uterine Inversion;" a prize of 2,500 francs to Dr. A. Mairet, of Montpellier, for his work "On Melancholic Dementia. The sum of 6,000 francs, a portion of the "prix Orfila," was divided between MM. Pierre, Rondeau, Gedeon, Meillére, and Alfred Houdé, pharmaciens of Paris, for their works on "Toxicological Subjects." The prix Saint-Paul, of the value of 25,000 francs, which is to be competed for without distinction of nationality or profession, is to be awarded to the candidate who will have discovered a remedy for diphtheria which, in the opinion of the Academy, would be considered the most efficacious for that malady. No work was considered worthy of the prize, but by way of encouragement rewards have been distributed as follows: Five hundred francs to Dr. G. Tedoldi, of Castel-Ariol, for his memoir, which contains a very good bibliographical and historical account of diphtheria and of its treatment by large doses of quinine; 500 francs to Dr. Otto Siefert, of Würzburg, for his memoir on the "Treatment of Diphtheria by Quinoline;" 500 francs to Dr. Lamarre, of St. Germain, for his memoir on the "Treatment of Diphtheria by Paintings with Petroleum Oil and Inhalations with the Vapors of Mineral Essence;" 500 francs to Dr. Delthil, of Nogent on the Marne, for his memoir on "The Treatment of Diphtheria by a Combustion of a Mixture of the Essence of Turpentine and Tar Gas." The sum of 2,000 francs is offered for the best observations on the part played by the first dentition in infantile pathology. The prize was divided as follows: One thousand francs to Dr. Séjourné, of Revin; 500 francs to Dr. Rousse, of Fontenay; 200 francs to Dr. T. Caradec, of Brest; 200 francs to Dr. Coriveaud, of Blaye. A prize of 1,000 francs was awarded to M. Lancry, Interne of Paris Hospitals, and 500 francs to Dr. Comby, for their work on "The Etiology and Prophylaxy of Scrofula in the First Period of Childhood." A prize of 2,000 francs each was awarded to Drs. Strauss and Roux for their scientific researches during the recent epidemic of cholera at Toulon. Dr. A. J. Martin received a prize of 4,000 francs for his work on "Foreign Civil Sanitary Administration."

The readers of the JOURNAL, and medical men everywhere, will regret to learn of the death of Dr. Noel Gueneau de Mussy, which has just occurred. His death is a great loss to French medicine, and indeed to scientific medicine generally. He was a most laborious and conscientious worker, and a cultivated gentleman. His writings on medical subjects are too well known to need more than a passing reference at present. A. B.

DOMESTIC CORRESPONDENCE

NEW YORK LETTER.

(FROM OUR OWN CORRESPONDENT.)

The Meeting of the American Climatological Association; Climatic Treatment of Phthisis; Sanitaria —Contagious Ophthalmia—Long Island College Hospital—The Manhattan Hospital.

The second annual meeting of the American Climatological Association, which was held in this city on the 27th and 28th of May, was very interesting and successful, and much good work was accomplished by it. The address of the President, Dr. Loomis, which was admirable, was largely devoted to the climatic treatment of phthisis. It was absurd, in his opinion, to claim for any climate that it exerted a specific influence upon the disease; but localities where there was the greatest purity of atmosphere, and consequent freedom from germs, were undoubtedly the best for the consumptive. It was universally recognized that a moist atmosphere favored phthisis, and a moist atmosphere was one laden with germs. In this connection he referred to several cases apparently indicating that the tubercle bacillus disappeared from the sputa of the phthisical patient in consequence of the change to an atmosphere unfavorable to the bacillus and where the condition of the patient improved.

In the course of his remarks he advocated the building of sanitaria in proper localities, in various parts of the country, for the benefit of those in straitened circumstances, and by way of illustration gave an interesting account of the sanitarium, on the cottage plan, now in successful operation in the Adirondacks. The expenses at present were $5 a week, and quite a number of patients had already been greatly benefited by a sojourn there. When any place was found to be advantageous by the consumptive, he thought it was advisable that he should remain there until well advanced towards recovery or until the recovery was complete. On the evening of the second day of the meeting a reception and collation were given the Association at the University Club Theatre.

At the meeting of the Academy of Medicine held June 1, Dr. Richard H. Derby presented, with some very pertinent remarks, a report on contagious ophthalmia in some of the orphan asylums and residential schools of New York city, which showed a very lamentable state of affairs. He had made a personal examination of the eyes of all the children in no less than twenty-four of these institutions, and in the paper gave a synopsis of the condition found in each of them. Altogether he examined 7,440 children, and of these, as many as 1,428, or 19-19 per cent., were suffering from blennorrhœal conjunctivitis or its complications. In the first of the asylums which he visited he found 24.5 per cent. of the inmates affected with contagious eye disease, in the second, 30.7 per cent., and in the third, actually over 49 per cent.—93 children out of 188. When we reflect upon the serious consequences that must inevitably result from this vast amount of ophthalmia, the total blindness or

permanent injury of the eyes of these poor children, it is no wonder that the author spoke with some feeling when he called upon the Academy of Medicine to undertake a movement for the suppression of this frightful evil. Among the suggestions which Dr. Derby made were, that all institutions designed for the care of children should be required by law to provide every possible precaution against the spread of contagious eye disease; that they should be required at stated intervals to make reports to the Board of Health of the number of inmates affected with such disease; that they should be required to strictly isolate all children thus affected; that they should be required to provide adequate treatment for every case, including not merely local treatment, but hygienic and general; and that they should be required to take exactly the same precautions to prevent the spread of contagious ophthalmia as of such diseases as scarlet fever and diphtheria. He advocated also the establishment of a special hospital for the reception of such cases, as it was not desirable to take them into the regular eye hospitals, on account of the danger of contagion, and as they could no doubt be treated to greater advantage in such a hospital than in the institutions to which they belonged.

In the discussion which followed, Dr. D. B. St. John Roosa argued in favor of such a special hospital. The cost to the state, he said, of a man with poor eyes, or altogether blind, was a very serious matter, and the reduction of the pauper class was certainly important enough to engage the attention of the political philanthropist.

Dr. C. R. Agnew said that he did not like the idea of entrusting such a matter to the state; but there was a limit to individualism and the power of voluntary efforts. It seemed evident to him, therefore, that there was need of some legislation. He believed in agitation of a subject, and it was his opinion that by judicious efforts of those specially interested in the matter, the public mind could readily be brought to realize the necessity for the adoption of active measures for repressing the existing evil. He thought that before an institution for the care of children should be granted an incorporate existence, it should place on file a certificate to the effect that it had ample provision for the satisfactory maintenance of a certain number of inmates, with a detailed statement of its capacity and accommodations, and that under no circumstances whatever should it be permitted to receive any larger number of children than that specified in such certificate. It was horrifying to think of actually making six or seven hundred paupers in the residential schools of a city like New York, and it was high time that something was being done to prevent the longer continuance of the present state of affairs. It was his opinion that the vast majority of children who contracted chronic conjunctivitis in these institutions never recovered from it, and in after life they would constitute an immense body of drifting and despairing sufferers, in many instances wandering aimlessly about from one dispensary to another. A few years ago it used to be the boast of Fourth of July orators that there were no indigenous paupers in this country; but all this was changed, and there was now unquestionably a large amount of indigenous pauperism here.

Dr. Jacobi expressed the opinion that this disease could never be eradicated from an institution in which it had occurred as long as that institution remained in the same spot and there were still children living in it. All these schools and asylums were no doubt meant to do good; but in reality they accomplished much harm. Outbreaks of ophthalmia were liable to occur, notwithstanding all the measures that could be adopted to prevent them, just as scarlet fever was liable to break out among the children in them. As a rule, diseases like measles and whooping-cough were not of a very serious character in private families; but in these institutions measles and whooping-cough were often very fatal or attended with grave consequences, and very frequently phthisis was found to be the result of one of these diseases contracted in early life in such an institution. If, therefore, the object contemplated was simply to add another to our already numerous hospitals, he did not think much good would be accomplished by the present agitation. In such a hospital for the treatment of contagious eye diseases he could not doubt that many cases which were mild when admitted would assume a serious type, and that the danger would be increased, instead of diminished, by placing children in it. The question must come up here, as it had already done all over Europe, whether these institutions for children ought to be multiplied at all; and for his part he felt convinced that it would be a benefit to the community at large if these public asylums and residential schools were done away with altogether. If the same money now expended for the erection and maintenance of splendid buildings and the same care now bestowed upon their management, were given to farming out the children in a judicious manner, he believed, we would be much better off.

At the conclusion of the discussion, Dr. Agnew presented some resolutions to the effect that it was necessary and expedient to take steps to restrain the spread of contagious ophthalmia, and that the Council of the Academy should, in connection with the State Board of Charities, the Society for the Prevention of Cruelty to Children and other charitable institutions, appoint a joint committee to devise some plan for the prompt and efficient accomplishment of this object.

The annual commencement of the Long Island College Hospital was held on June 3, when the degree of M. D. was conferred on 47 graduates.

The corner-stone of the new Manhattan Hospital, at 131st Street and 10th Avenue, was laid May 23 with appropriate ceremonies. The hospital is being built by the Manhattan Dispensary Association, which has maintained a dispensary service in Harlem ever since 1862, but has not hitherto engaged in hospital work proper. Only one wing of the building, as eventually contemplated, will be erected at present, and it is expected that it will be ready for occupancy by the 1st of October. The nearest emergency hospital up to this time has been the city branch hospital at 99th street, and the necessity for one further up

town became so great, especially in view of the rapid increase of the population in this growing district, that the gentlemen interested in the Manhattan dispensary raised a subscription for the institution.

P. E. P.

THE TREATMENT OF DIPHTHERIA.

Dear Sirs:—Having been much interested by Dr. Nunn's article on the treatment of diphtheria, published in the JOURNAL of June 13, I wish to testify to the good effects of the biniodide of mercury in my hands. For the past six years I have depended upon it entirely for constitutional treatment, and in that time have not had a single case of paralysis, and a much smaller proportion of fatal cases, than with any other treatment I have ever used. For local treatment I have generally used tr. ferri chlor., 1 part to 2 parts of water, applied every two or three hours with a brush. Respectfully,

D. T. BROWN, M. D.,

Michigan City, Ind., June 15, 1885.

BOOK REVIEWS.

CLINICAL STUDIES ON DISEASES OF THE EYE, INCLUDING THOSE OF THE CONJUNCTIVA, CORNEA, SCLEROTIC, IRIS AND CILIARY BODY. By DR. FERDINAND RITTER VON ARLT, Professor of Ophthalmology in Vienna. Translated by LYMAN WARE, M.D., Surgeon to the Illinois Charitable Eye and Ear Infirmary; etc., Chicago. 8vo., pp. 325. P. Blakiston, Son & Co., Philadelphia, 1885.

It has been a pleasure to have such an admirable translation of Dr. Ferd. Ritter von Arlt's classic work entitled "Klinische Darstellung der Krankheiten des Auges, etc.," presented to us for review. The original, which was first published in Vienna as late as 1881, and which has ever since its appearance been looked upon by the ophthalmological world as a safe and valuable guide to the correct understanding of those diseases of the eyes which can only be mastered by many years of careful study in situations presenting unexceptional clinical advantages, was written under unusually favorable conditions. The author, a man of great experience, who so happily possesses a discernment ever ready to grasp any distinctive features that might arise, combined with so rare a judgment as to be able to successfully combat any injurious symptom by therapeutical means, has in this work given the results of a long series of clinical researches extending over a period of nearly fifty years. That such an exposition in book form should possess for both the general practitioner and the special scientific observer, a worth that is almost incalculable, is at once apparent; and yet, by reason of the want of an English translation, the value of the work has remained a locked secret to many English and American professional men, who are unable to understand the language of the original. It is for these that Dr. Lyman Ware, of Chicago, has given us an English version of this almost unique addition to ophthalmic literature. He is to be congratulated upon the success of his undertaking. After careful comparison with the original, we find that the American edition, which is published in the usual good style of the Blakistons, is remarkably free from interpolations (a fault too often found in the average translation) and unexceptionably clear of literalism. The German sentences have been anglicized into smoothly running English expressions of synonymous signification. A few useful additions in reference to the value of jequirity and cocaine have been added.

We can unreservedly offer and recommend Dr. Ware's translation of von Arlt's work upon the "Clinical Studies on Diseases of the Eye" to those of our colleagues who are compelled to restrict their studies upon the subject to the English language, as one to be relied upon; it being a true and conscientious exponent of the views and opinions expressed by the great and distinguished author.

OFFICIAL REGISTER OF PHYSICIANS AND SURGEONS WHO HOLD CERTIFICATES FROM THE BOARD OF EXAMINERS OF THE MEDICAL SOCIETY OF THE STATE OF CALIFORNIA, January 31, 1885. To which has been appended a complete List of those who hold Certificates from the Homœpathic and Eclectic Boards; also, A Complete List, as far as procurable, of all Persons Practicing Medicine in this State without a Certificate from either of the Boards. Second Edition; revised and published by the Board. R. H. PLUMMER, M.D., Recording Secretary, San Francisco.

Beyond the fact that the verb "practise" is persistently misspelled in this pamphlet (and the additional fact that *either* is wrongly used on the title-page), this Official Register seems to be all that can be desired; and when it is remembered that the compilation of it has involved an enormous amount of labor, it is only justice to Dr. Plummer to say that he is deserving of very great praise.

It does not appear from this pamphlet why there are three Boards of Medical Examiners for the State of California; nor does it appear credible that with three Boards there should be 482 illegal practitioners in the State. It is reasonable to infer that the State would be fairly overrun with illegal practitioners if there were two or three more Boards. In addition to the list of practitioners in the State, the Register also contains a copy of the laws regulating the practice of medicine in California, with the decisions of the Supreme Court in two cases, the minimum requirements for a course of medical instruction, the Code of Ethics, the San Francisco and Chicago fee bills, a directory of medical colleges, hospitals, dispensaries, medical societies, asylums and boards of health in the State, and a list of the California mineral springs; making it a very convenient reference book. We hope that when the third edition is issued the list of illegal practitioners will be somewhat shorter. The Register states that the illegal practitioners are liable to prosecution and fine upon the complaint of any citizen; though we are at a loss to know why the Examining Boards should wait for such complaint. It would seem that one of the three Boards might take the initiative in complaining.

ASSOCIATION ITEMS.

INTERNATIONAL MEDICAL CONGRESS COMMITTEE.

When that part of this Committee consisting of one from each state and territory was announced in the meeting in New Orleans, Connecticut, North Carolina, Oregon and Wyoming Territory were omitted.

The President, J. S. Lynch, of Baltimore, who occupied the chair at the time, has supplied the defect by appointing Dr. W. C. Wile, of Sandy Hook, Connecticut; Dr. N. J. Pittman, of Tarboro, N. Carolina; Dr. E. C. Fraser, of Portland, Oregon, and Dr. J. J. McAchran, of Laramie City, W. T., as members of the Committee.

The whole Committee is expected to meet at the Palmer House in this city (Chicago) on the 24th inst., in accordance with the official notice published in our last week's issue.

MISCELLANEOUS.

THE IMPORTATION OF INFECTED RAGS.—The Secretary of the Treasury has issued the following circular: "Whereas it has been conclusively shown to the department that, under existing laws, no general regulation can be legally framed whereby the disinfection of old rags can be accomplished in foreign ports to the satisfaction of the several health authorities, therefore it is ordered: 1. That all circulars of this department concerning the disinfection of imported old rags are hereby revoked, and that old rags hereafter imported from foreign countries shall be admitted to entry at the Custom-House only upon the production of permits from the health officers at the ports of importation duly authorizing the landing of the same. 2. Vessels carrying old rags arriving at any United States quarantine will be detained by the quarantine officers, and held subject to the order of the proper health authorities at the port of destination."

MASSACHUSETTS MEDICAL SOCIETY.—At the one hundred and fourth annual meeting of this Society, held in Boston June 9 and 10, the following officers for the ensuing year were elected: President, Charles D. Homans, of Boston; Vice-President, Dr. Munsell, of Harwich; Treasurer, Dr. Frank W. Draper, of Boston; Corresponding Secretary, Dr. Charles W. Swan, of Boston; Recording Secretary, Dr. Francis W. Goss, of Roxbury; Librarian, Dr. Edwin H. Brigham, of Boston; Orator, Dr. R. M. Hodges, of Boston; Anniversary Chairman, Dr. Harvey, of Westbury. The various committees chosen were the same as last year.

COLLECTIVE INVESTIGATION IN RUSSIA.—The medical societies of Russia formed, at a recent meeting held in St. Petersburg, a Collective Investigation Committee for Russia, which will communicate with the English Committee. Professor Lesshaft and Dr. Monastyrski are the Secretaries. The subjects for collective investigation will be those proposed by the International Committee, and with the same tables of questions, namely: rhachitis, calculus, typhus, exanthematicus, acute rheumatism, and diseases of the age of school-attendance.

THE COMMA-BACILLUS: PROPOSAL FOR A COMMISSION OF INQUIRY.—MR. W. WATSON CHEYNE, in a note to the *British Medical Journal* of May 30th, thinks that the only way in which the questions in dispute between himself and Dr. Klein can be decided, is by repeating the experiments before a competent commission; and says:

The subject is of such immense importance that it certainly should not be allowed to rest in its present unsatisfactory state. There are four statements made by Dr. Klein which ought, I think, on account of their importance, to be submitted to a commission.

1. Dr. Klein states that in the cæcum of healthy guinea-pigs the "comma-bacilli of Koch" are present. I have investigated this matter, and cannot confirm this statement. Let us repeat the experiment side by side before the commission.

2. Dr. Klein states that after isolation by ligature of a loop of intestine in monkeys, and injection of sulphate of magnesia into the part between the ligatures, comma-bacilli appear in the contents of this part which he is "unable to distinguish from the choleraic comma-bacilli." I have not repeated this observation; let it be done before the commission.

3. Dr. Klein states that the comma-bacilli of the saliva can be cultivated in neutral jelly, and, after acclimatization in this medium, can be made to grow in the same material as is used for the cultivation of the cholera-bacilli, and that then the cultivation of the two organisms are identical in every respect. I cannot confirm the statement that the comma-bacilli of the saliva can be grown in neutral jelly. The observation ought to be repeated.

4. Dr. Klein makes various statements with regard to the behavior of cholera-bacilli with acids which are in apparent contradiction to my results, and which it would be well to test before the commission; not that this is an essential point, but it would serve to illustrate the methods employed, more especially the sufficiency of the control-experiments.

It would also be well to repeat some of Dr. Koch's recent experiments on animals, in order to give Dr. Klein the opportunity of proving satisfactorily, by control-experiments, that the results are due to septicæmia or to the surgical interference.

As regards the conditions under which the experiments should be done, it seems to me essential that, having regard to Mr. Dowdeswell's experience, they should be carried out in a laboratory in which cultivations of cholera-bacilli have not previously been made; that the apparatus used—more especially the syringes, needles, etc.—should be new; that every step we take should be in the presence, and subject to the criticism, of each other and the members of the commission. The commission would not, of course, be asked to express any opinion as to Dr. Koch's views, but merely to state the facts which they see.

THE NEW YORK COLLEGE OF MEDICINE AND SURGERY.—The bill for the incorporation of this proposed pernicious addition to the medical colleges of New York has been vetoed by the Governor of that state.

CHARITY-MATCHES OF THE SCOTTISH FOOTBALL ASSOCIATION.—Last year, says the *British Medical Journal*, of May 30, we drew attention to the very commendable practice of the Scottish Football Association, by which the surplus funds resulting from the drawings at the various Charity Cup matches were divided amongst several of the charitable institutions of Glasgow. This plan has been followed again this year, and different charities have been benefited to the extent of £520; the Royal and Western Infirmaries heading the list with £100 each. During the ten years that these matches have been instituted, no less a sum than £5,240 has been disbursed by the Association from this source.

AMERICAN LARYNGOLOGICAL ASSOCIATION.—The seventh annual congress of the American Laryngological Association will be held in Abstract Hall, Lafayette Avenue, Detroit, Mich., June 24, 25, and 26, 1885. The profession is cordially invited to attend the sessions of the congress.

THE REPORTING OF CONTAGIOUS DISEASES.—A SUGGESTION TO THE BOARD OF HEALTH.—Dr. J. W. Brannan, of New York, makes the following very sensible suggestion: "I see by the last *Record* that the Board of Health complain that many physicians do not report promptly their cases of contagious disease. I would suggest to our Health Board that if they would simplify the work of reporting, there would probably be less neglect of the regulations on the part of physicians. In Boston all physicians are provided by the local authorities with postal cards, on one side of which is printed the address of the Board of Health, and on the other very much the same form of words that is found on slips furnished to physicians in New York. In case of contagious disease, the blank spaces in the form are filled out, the card is dropped into a post-box, and the matter is over. I commend the above plan to our Board of Health, that they may secure greater efficiency of service."—*Medical Record*, June 13, 1885.

DANGERS FROM INFECTED STRAW.—A dispatch from New York, of June 16, states that the dealers in hay and straw propose to begin active measures against the steamship companies for their neglect to destroy the straw bedding used by the steerage passengers unless the companies mend their ways very soon. A leading hay dealer says: "The public does not know its danger from this source. When the Pilot Commissioners forbade the further throwing of the straw in the harbor, the companies invoked the aid of licensed vendors to help them get rid of the old bedding. It was turned out on the piers, and these vendors were either made a present of it or bought it at a low price. They now take it to Hoboken and Jersey City, where it is retailed and sold to crockery dealers and others for packing purposes. We are taking all sorts of precautions against the spread of cholera, and here, right under our noses, a stuff which easily carries infection and which is likely to have disease germs within it, is not only permitted to be landed to the amount of 100 tons weekly, but is scattered broadcast all over the country, carrying possibly pestilence with it."

CHOLERA IN SPAIN.—A dispatch from Madrid, of June 16, states that in the Province of Castellon de la Plana there were eighty cases of cholera and sixty deaths on June 15. Thirty thousand people have fled from Murcia owing to the terrible dread of the disease which has taken possession of the people. In the city of Murcia alone, on June 15, there were, according to the latest rumors, sixty-four new cases of cholera and thirty-nine deaths. In the Province of Murcia outside of the city there were fifty cases of cholera and sixteen deaths within the same period. The *Official Gazette* confirms the statement that cholera is raging in the Provinces of Valencia, Castellon de la Plana, and Murcia, and in the city of Madrid. The Opposition newspapers, on the contrary, deny the existence of Asiatic cholera in the city of Madrid.

THE ITALIAN MEDICAL ASSOCIATION will hold its eleventh annual meeting in Perugia in September, 1885, at which there will be an exhibition of medicinal articles, and of surgical and sanitary instruments and appliances.

DEATH OF DR. THORBURN.—The English journals announce the death of Dr. John Thorburn, Professor of Obstetric Medicine at the Owens College and Victoria University, Manchester. His "Practical Treatise on the Diseases of Women" was published just before his death. He was instrumental in the foundation of the British Gynæcological Society.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM JUNE 6, 1885, TO JUNE 12, 1885.

Major B. E. Fryer, Surgeon, U. S. Army, ordinary leave of absence extended six months, from July 1, 1885, on surgeon's certificate of disability. (S. O. 129, A. G. O., June 6, 1885.)

The order directing Major P. J. A. Cleary, Surgeon U. S. A., to change station from Ft. Union, N. M., to Ft. Lyon, Col., is revoked. (S. O. 81, Dept. Mo., June 8, 1885.)

Captain Charles Richard, Assistant Surgeon, granted leave of absence for three days. (S. O. 125, A. G. O., June 5, 1885.)

First Lieutenant C. N. B. Macauley, Assistant Surgeon, relieved from duty at Ft. Sisseton, D. T., and ordered for duty at Ft. A. Lincoln, D. T. (S. O. 61, Dept. Dak., June 5, 1885.)

OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF MEDICAL OFFICERS OF THE U. S. MARINE HOSPITAL SERVICE FOR THE WEEK ENDED JUNE 13, 1885.

Walter Wyman, Surgeon, to proceed to New York, N. Y., and assume charge of the Service, relieving Surgeon Sawtelle, June 8, 1885.

Banks, C. E., Passed Assistant Surgeon, granted leave of absence for thirty days, June 12, 1885.